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Correction: May, 1915, p. 216, line 2, delete " in." The sentence when corrected should read: " One mental process is the meaning of another mental process if it is that other's context."

VOL. XXII. No. 6 November, 1915

THE PSYCHOLOGICAL REVIEW

REACTIONS TO THE CESSATION OF STIMULI AND THEIR NERVOUS MECHANISM

BY HERBERT WOODROW

University of Minnesota

The study of reactions to the cessation of stimuli, although hitherto largely neglected, nevertheless presents a number of points of genuine scientific interest. Reactions to the cessation of stimuli, or cessation reactions, are the same as regards the reaction movement as ordinary reactions to the beginning of stimuli, or beginning reactions. While, in the case of beginning reactions, we have energy acting upon some receptor of the individual's nervous system, and producing a motor response, in the case of cessation reactions, we have merely the discontinuance of energy, which, on the reflex theory, would readily account for the discontinuance of the motor response. But reference to more complex processes than mere reflex conduction is necessary in order to understand how the mere discontinuance of energy can produce a positive reaction similar to any produced by the application of energy; and yet, if cessation reactions are found to be as quick as beginning reactions, the same complexity of mechanism must be presumed for both.

Another point of interest, in connection with cessation reactions, is the question whether the relation of intensity of stimulus to reaction time is the same as in beginning reactions, where the times are longer with weak stimuli than with strong. This relation might be reversed if the reason for the long reaction times with the weak stimuli was that in such cases a weak nervous current was meeting with great resistance which it took a long time to overcome: for then the cessation of the

weak nervous current could not also meet with great resistance, but, on the other hand, the weaker the current, the more its cessation would be favored.

That cessation reactions may throw some light upon the explanation of the differences in reaction time which occur with variation in the mode of stimulus has already been recognized.[1] The fact, that with moderate intensities, the reaction time to sound is ordinarily shorter than that to light is commonly explained on the ground that the stimulation of the ear by sound, a mechanical process, takes less time than the stimulation of the eye by light, a chemical process. That this explanation is as yet a matter of speculation must be admitted, as has been pointed out by Dunlap and Wells, who instigated a series of experiments designed to afford a more satisfactory explanation.[2] Wells, with this same problem in mind, conducted some experiments in which the reactions occurred upon the disappearance or occlusion of the stimulus. That is, the stimulus consisted in darkness, preceded and followed by illumination, with the subject reacting at the moment the darkness appeared. From his experiments, he concluded that the lag in the sensory process in the case of vision cannot account for as much as 10 σ of the lengthening of the visual reaction time beyond that of the auditory. The argument, by which he arrives at this conclusion, need not here be analyzed, as it is not based so much upon the reaction times to the disappearance of the stimulus, as upon the fact that an interruption of the light for only 10 σ was found to be plainly perceivable.[3] As regards the reaction time to the disappearance of the light stimulus, Wells concludes that it differs little from that to the appearance of the light. This conclusion is supported by a large number of sensory reactions, and is in conformity with that indicated by the data presented below.

Another question concerning cessation reactions is whether the after-image has anything to do with determining the

[1] Wells, G. R., 'The Influence of Stimulus Duration on Reaction Time,' Psychol. Monog., 5, 1913.

[2] 'Some Experiments with Reactions to Visual and Auditory Stimuli,' Psychol. Rev., 1910, 319.

[3] *Op. cit.*, 65.

reaction time. Many other questions also suggest themselves, but the problems that have been referred to are sufficient to indicate the nature of the scientific interest of the present investigation.[1]

In the case of all the reactions here reported, the instructions called for a reaction which would ordinarily be regarded as a form of "motor" reaction. Reactions are usually divided into two main groups, those in which, during the preparatory interval, the attention is mainly directed to the stimulus, called sensory, and those in which it is focused primarily upon the reaction movement, called motor. A more important distinction, however, at least from the point of view of reaction *time*, is that between reactions with instruction to react as quickly as possible, and reactions without such instruction.[2] In the present instance, the subjects were always instructed to make every reaction as quickly as they possibly could. Nothing was said about the direction of attention, but the subjects' introspections indicated that their attention was, primarily, neither upon the stimulus or its image, nor upon the reaction movement or its image, but upon the idea of reacting as quickly as possible. Just how this idea was carried in the subjects' minds, that is, in what imagery, or whether in any imagery, I am unable to conclude from the introspective data. No attempt was made at elaborate systematic introspection.

Apparatus.—The reaction times were measured by means of a Hipp's chronoscope. The chronoscope circuit and the stimulus circuit were separate, but by means of a double switch both circuits could be closed simultaneously. The stimulus, for light reactions, consisted in the illumination of a Geissler's tube, while for sound reactions, the stimulus was the vibration of a telephone receiver. For beginning reactions, both the chronoscope and stimulus circuits were closed at exactly the same instant by means of the double switch. Each side of this switch was provided with a platinum wire which sank into an adjustable cup of mercury when the experimenter tapped upon the switch handle. For cessation reactions, the stimulus circuit was arranged so that it was closed when the handle of the switch was raised, and broken when the handle was tapped down. Thus, in this case, the chronoscope circuit was closed as the stimulus circuit was broken, though there was an exceedingly slight time between the breaking of the latter and the closing of the former. This time, when measured, was found to

[1] The general subject of cessation reactions was first suggested to me by Professor H. C. Warren of Princeton, in 1907. The results here reported on sound were presented in full in a paper read in April, 1912, before the Minnesota Psychological Conference.

[2] See Woodrow, 'The Measurement of Attention,' Psychol. Monog., 1915, Chap. II.

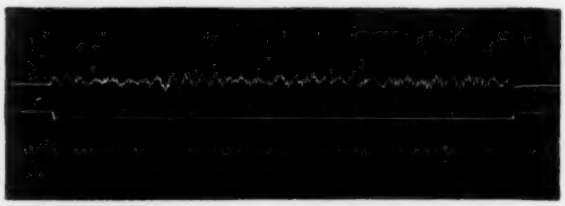
vary from 0 to 3 σ, but is regarded as a constant error of 2 σ, and added to all averages of cessation reaction times.

The chronoscope circuit included besides the chronoscope and half of the double switch already mentioned, the subject's reaction key, a battery of Edison primary cells of 12 volts, and a Wundt's fall-hammer. The chronoscope was controlled before and after each 50 reactions, by means of the fall-hammer, and the fall-hammer itself was tested each day with a 250 d.v. fork.

The stimulus circuit, when arranged for light reactions, included, as already stated, a Geissler's tube. This tube was suspended before an oblong aperture in a dark box, a few feet in front of and slightly below the subject's eyes, as he sat in the dark-room at his reaction key. The Geissler's tube was actuated by an inductorium placed in the experimenter's room. The buzzer of the inductorium was kept going continuously during the experiment, but only the secondary circuit, which included the Geissler's tube, passed through the operator's double switch, and so the tube was luminous only when the secondary circuit was closed at this switch. In order to weaken the intensity of the stimulus, no change was made in the electrical circuits, but a ground glass covered with white paper was placed over the aperture of the dark box containing the Geissler's tube.

In the case of sound reactions, the sound used as a stimulus was that made by a telephone receiver, through which there passed a current which was interrupted both 50 and 250 times per second, by two electro-magnetic tuning forks. All the current passed through the forks, but the telephone was in a shunt circuit. Consequently, the forks ran continuously, whereas the telephone ran only while its circuit was closed at the experimenter's double switch. By decreasing the resistance in parallel with the telephone, the loudness of the telephone could be decreased.

The reason for running the telephone current through both a 50 and a 250 fork was that only in this way could the click which followed the break of the current be eliminated. This method is noted by Pillsbury in his discussion of 'Methods for the Determination of the Intensity of Sound,'[1] and is one that I have long employed.[2] Records of the vibration of the telephone plate were obtained by attaching a light glass pointer to the plate and having this pointer mark upon a smoked drum alongside a Pfeil time marker placed in the chronoscope circuit. A sample of these records is here reproduced.



The vibrations of the 250 fork are seen superimposed upon those of the 50 fork. The record shows that the sound started in at its maximum intensity with the first

[1] 'Report of the Committee of the American Psychological Association on the Standardizing of the Procedure in Experimental Tests,' Psychol. Monog., 1, 1910. Pillsbury writes as follows: "Dr. Shepard, working in my laboratory, found that the click could be lessened to a point of not being noticed if two tones were superimposed upon the telephone. He used the commercial current of 60 cycles and a 250 d.v. fork. The physical basis for the effect is obscure, but the empirical effect is obvious."

[2] See 'A Quantitative Study of Rhythm,' *Archives of Psychol.*, 12, 1909.

vibration of the telephone plate, and that it stopped as suddenly as it began. It will be noted that no large vibrations follow the breaking of the circuit as they would if there were a click. A number of careful observers agreed that there was no noticeable click at either the closing or the opening of the circuit, thus confirming the objective record.

Sound reactions were taken with three different intensities of stimulus, called medium, weak and liminal. Physical measurements of the intensity were not made, but care was taken to keep the current the same throughout the work with any one intensity. The reaction times themselves offer a sufficient index of the intensity. The intensity given in Table I. as liminal, in reality exceeded the true limen by an extremely small amount. It was determined as follows: Four series of minimal changes in intensity were used, two of which passed from an audible to an inaudible sound and two from an inaudible to an audible one. This procedure was just what one might employ to determine the sensation threshold. The sound was then placed not at the average liminal intensity obtained, but at the lowest intensity which did not fail to produce a sensation in any of the four series.

Since an object of the present study is a comparison of cessation reactions with beginning reactions, both kinds of reactions were always taken at each sitting of one hundred reactions. On alternate days the middle fifty reactions were cessation reactions and the first and last twenty-five were beginning reactions; while on the remaining days the middle fifty were beginning reactions and the first and last twenty-five were cessation reactions. In the tables below, averages for beginning and cessation reactions given on the same line of the table were obtained at the same sitting. Each average is the average of fifty reactions. No reactions were thrown out on account of their deviation from the average, except in the case of the liminal sound. However, all reactions which, by a prearranged signal, the subject indicated were mistakes, as well as all cases of error of manipulation on the part of the experimenter, were rejected. In the case of the liminal sound there were a number of cases where no reaction at all occurred and some that were very long, from 1,500 to 5,000 σ. These long reactions occurred both in the case of beginning and

cessation reactions, but all reactions over 1,500 σ were not counted. The total number of reactions to sound and light stimuli reported in the following tables is 10,000.

As a warning signal, in the case of beginning reactions the click of an electric sound-hammer was used, and given always two seconds before the reaction stimulus, the interval being indicated to the experimenter by means of a pendulum. In the case of cessation reactions the beginning of the stimulus itself acted as the warning signal. The cessation of the stimulus always occurred two seconds after its beginning.

Five subjects were used, three of which, subjects *Ww*, *Ht*, and *St* were quite practiced in beginning reactions before the work began, while the other two, subjects *Vs* and *Sz*, were altogether unfamiliar with work in reaction time.

The results obtained for reactions to sound and to light are presented in Tables I., II. and III. These tables show the results of each sitting in the order in which they were obtained. The results are summarized in Table IV. In this latter table, the mean variation given is the average mean variation for a series of fifty measurements.

The conclusion to be drawn is clear and simple. In the case of sound and light reactions, there is no appreciable difference between reaction time to the beginning of a stimulus and reaction time to its cessation, no matter what the intensity of the stimulus, and no matter what its mode. In view of the variability in reaction times, such a close correspondence between the cessation and the beginning reaction times as shown by Tables I. to IV. could not be regarded as a reasonable expectation unless the true values of both were in all cases substantially the same.

It is to be regretted that results with other senses than light and sound were not obtained. Difficulties of technique were largely responsible for limitation of the work to these modalities. Some reactions to touch were taken, however, using an intermittent current passing through two fingers of the left hand, each finger being placed in a salt-water electrode; but so much difficulty was experienced as a result of sensory adaptation and the after-tingling of the fingers, that

TABLE I

SOUND REACTIONS

N, for each av. = 50. Total No. of reactions = 4,000.

Intensity	Subj.	Series	Beginning Reactions		Cessation Reactions	
			Av.	*M. V.*	Av.	*M. V.*
Medium	*Ht*	I.	115	*11*	121	*15*
"	"	II.	113	*19*	125	*15*
"	"	III.	121	*20*	128	*17*
"	"	IV.	136	*20*	127	*15*
"	"	V.	123	*13*	115	*15*
"	"	VI.	116	*14*	121	*11*
"	"	VII.	106	*12*	113	*15*
"	"	VIII.	122	*15*	120	*13*
"	"	Av.	**119**	**16**	**121**	**15**
"	*Vs*	I.	143	*17*	144	*16*
"	"	II.	123	*18*	142	*20*
"	"	III.	141	*18*	155	*18*
"	"	IV.	135	*15*	147	*21*
"	"	V.	140	*20*	136	*14*
"	"	VI.	145	*12*	144	*17*
"	"	VII.	132	*15*	138	*16*
"	"	VIII.	137	*17*	140	*14*
"	"	Av.	**137**	**17**	**143**	**17**
"	*Sz*	I.	146	*20*	157	*24*
"	"	II.	143	*17*	155	*12*
"	"	III.	162	*20*	143	*22*
"	"	IV.	137	*23*	151	*18*
"	"	V.	140	*14*	152	*23*
"	"	VI.	154	*22*	136	*17*
"	"	VII.	147	*26*	147	*25*
"	"	VIII.	157	*14*	144	*25*
"	"	Av.	**148**	**19**	**148**	**21**
Weak	*Ht*	I.	172	*34*	170	*44*
"	"	II.	202	*27*	190	*26*
"	"	III.	175	*18*	170	*27*
"	"	IV.	185	*24*	201	*36*
"	"	Av.	**184**	**26**	**183**	**33**
"	*Vs*	I.	188	*18*	186	*20*
"	"	II.	196	*28*	155	*32*
"	"	III.	160	*17*	155	*20*
"	"	IV.	150	*18*	171	*17*
"	"	Av.	**174**	**20**	**167**	**22**
"	*Sz*	I.	215	*31*	226	*35*
"	"	II.	198	*34*	204	*27*
"	"	III.	222	*31*	223	*40*
"	"	IV.	201	*36*	218	*29*
"	"	Av.	**209**	**33**	**218**	**33**
Liminal	*Ht*	I.	829	*146*	682	*119*
"	"	II.	728	*114*	807	*147*
"	"	Av.	**779**	**130**	**745**	**133**
"	*Vs*	I.	755	*137*	735	*177*
"	"	II.	995	*315*	909	*359*
"	"	Av.	**875**	**226**	**822**	**268**

TABLE II

REACTIONS TO BRIGHT LIGHT

N, for each av. = 50. Total No. of reactions = 3,600.

Subj.	Series	Beginning Reactions		Cessation Reactions	
		Av.	*M. V.*	Av.	*M. V.*
Ww	I.	160	*15*	152	*17*
"	II.	142	*11*	147	*16*
"	III.	154	*13*	160	*16*
"	IV.	140	*17*	152	*21*
"	V.	169	*19*	153	*13*
"	VI.	168	*24*	144	*13*
"	VII.	159	*17*	145	*16*
"	VIII.	140	*15*	157	*14*
"	Av.	**154**	**16**	**151**	**16**
Ht	I.	156	*18*	169	*18*
"	II.	163	*25*	158	*25*
"	III.	168	*19*	171	*12*
"	IV.	160	*21*	171	*15*
"	Av.	**162**	**21**	**167**	**18**
St	I.	190	*17*	187	*20*
"	II.	192	*22*	190	*13*
"	III.	187	*15*	180	*20*
"	IV.	178	*21*	192	*16*
"	V.	187	*22*	176	*26*
"	VI.	178	*19*	187	*16*
"	VII.	176	*11*	170	*22*
"	VIII.	178	*28*	193	*15*
"	Av.	**183**	**21**	**184**	**19**
Vs	I.	200	*24*	196	*21*
"	II.	207	*22*	177	*12*
"	III.	190	*18*	180	*13*
"	IV.	193	*14*	193	*18*
"	V.	194	*17*	192	*19*
"	VI.	179	*20*	178	*16*
"	VII.	186	*18*	180	*16*
"	VIII.	186	*21*	184	*20*
"	Av.	**192**	**19**	**185**	**17**
Sz	I.	196	*13*	200	*23*
"	II.	200	*23*	193	*27*
"	III.	207	*21*	193	*18*
"	IV.	177	*23*	201	*14*
"	V.	191	*25*	200	*22*
"	VI.	199	*21*	200	*23*
"	VII.	216	*28*	205	*23*
"	VIII.	221	*24*	212	*26*
"	Av.	**201**	**22**	**201**	**22**

the results cannot be regarded as reliable. The few that were obtained showed a marked prolongation in reaction time with decrease in the intensity of the stimulus, both with beginning and cessation reactions. The cessation reactions, however, were uniformly about 30 σ longer than the beginning reactions, which fact may be attributed to the after-tingling

of the fingers set up by the irritation of the electric current. Some experiments were also tried, in which the stimulus consisted in the fall and rise of an electric hammer arranged to strike the back of the finger. In this case, while the stimulus was adequate, one could not be certain that the

TABLE III

REACTIONS TO WEAK LIGHT

N, for each av. = 50. Total No. of reactions = 2,400.

Subj.	Series	Beginning Reactions		Cessation Reactions	
		Av.	*M. V.*	Av.	*M. V.*
Ht	I.	179	*35*	192	*28*
"	II.	196	*44*	178	*30*
"	III.	211	*37*	205	*26*
"	IV.	216	*34*	214	*22*
"	V.	204	*20*	194	*18*
"	VI.	221	*32*	217	*24*
"	VII.	211	*28*	217	*32*
"	VIII.	201	*33*	209	*28*
"	Av.	**205**	**33**	**203**	**24**
Vs	I.	221	*25*	230	*21*
"	II.	217	*23*	222	*15*
"	III.	252	*43*	214	*21*
"	IV.	240	*18*	234	*30*
"	V.	244	*22*	255	*21*
"	VI.	258	*29*	229	*18*
"	VII.	269	*19*	241	*22*
"	VIII.	241	*28*	243	*17*
"	Av.	**243**	**26**	**234**	**21**
Sz	I.	254	*27*	244	*15*
"	II.	239	*33*	250	*22*
"	III.	269	*30*	252	*31*
"	IV.	296	*22*	269	*29*
"	V.	284	*32*	268	*30*
"	VI.	258	*26*	255	*34*
"	VII.	264	*24*	228	*21*
"	VIII.	280	*27*	230	*23*
"	Av.	**268**	**28**	**250**	**26**

removal of the pressure was a change equivalent in intensity to its occurrence; but the results obtained in this case were to the effect that the beginning and cessation reaction times are equal. Thus, with one subject, the average reaction time for 100 reactions to the beginning of the stimulus, was 119 σ and for 100 reactions to its removal, 120 σ. With another subject, the average for 100 beginning reactions was 128 σ and for 100 cessation reactions with the same stimulus, 132 σ.

The simplicity of the conclusions drawn above from the results presented in Tables I. to IV. should not blind us to the important bearing that they have on current theories of the action of the nervous system. The explanation of the results here presented, by means of the theories of nervous action that are at present most in vogue, is extremely difficult,—so difficult, in fact, as to suggest that these theories are themselves incorrect, and that they must either be given

TABLE IV

A SUMMARY OF TABLES I., II. AND III

Mode	Intensity	Subj.	Beginning Reactions		Cessation Reactions	
			Av.	*Av. M. V.*	Av.	*Av. M. V.*
Sound	Medium	*Ht*	119	*16*	121	*15*
"	"	*Vs*	137	*17*	143	*17*
"	"	*Sz*	148	*19*	148	*21*
"	Weak	*Ht*	184	*26*	183	*33*
"	"	*Vs*	174	*20*	167	*22*
"	"	*Sz*	209	*33*	218	*33*
"	Liminal	*Ht*	779	*130*	745	*133*
"	"	*Vs*	875	*226*	822	*168*
Light	Bright	*Ww*	154	*16*	151	*16*
"	"	*Ht*	162	*21*	167	*18*
"	"	*St*	183	*21*	184	*19*
"	"	*Vs*	192	*19*	185	*17*
"	"	*Sz*	201	*22*	201	*22*
"	Weak	*Ht*	205	*33*	203	*24*
"	"	*Vs*	243	*26*	234	*21*
"	"	*Sz*	268	*28*	250	*26*

up or seriously modified. In particular, it can be shown that certain phenomena which it is customary to explain by reference to the latent period of sensory stimulation or by reference to certain hypothetical effects of the synapse, are not adequately explained thereby. Their explanation requires either a theory which is incompatible with the statement that the reflex is the type of all nervous activity, or else a theory which involves some modification of the ordinary concept of the reflex.

In the first place, let us consider the explanation of the increase in reaction time with decrease in the intensity of

stimulus. Piéron,[1] in a recent able discussion of this matter, concludes that the main factor, in determining the increase in reaction time, is the increase in the latent period of excitation of the first sensory neurone with decrease in intensity of stimulation. His discussion of other possible explanatory factors, however, is unprejudiced. He correctly regards as very doubtful the proposition that the rate of transmission of nervous energy along a neurone varies with the intensity of stimulation.[2] Likewise, and with equal correctness, he rejects the idea that the effect can be explained by reference to phenomena on the motor or centrifugal side of the process.[3] He does not deny, though, that when the excitation is weakened there may be an increase in the time of transmission from one neurone to another. He thinks, however, that the variations in time, due to this last-mentioned factor, are not very considerable, and that they do not suffice to explain the variations obtained experimentally. Piéron also raises the question whether the central or brain phase is not the seat of the principal variations. "La partie la plus longue, dans cette phase, correspond à la circulation de l'influx associatif dirigé, et sa brièveté dépend surtout de l'état d'attention, c'est-à-dire de l'aiguillage préalable qui assure la conduction de cet influx par les voies les plus rapides."[4] He argues, however, that since the reactions are always accompanied by a state of intense attention, the fact that a more intense

[1] 'Recherches sur les lois de variation des temps de latence sensorielle en fonction des intensités excitatrices,' *Année psychol.*, 1914, 17–96.

[2] In confirmation, see the following: Gotch, *Journ. of Physiol.*, 1902, 395. Koike, *Ztsch. f. Biol.*, 1910, 310. Lucas, *Journ. of Physiol.*, 1911, 46. Adrian, *Ibid.*, 1912, 389; 1913, 384.

[3] In this connection, one may well cite the work of Moore, 'A Study of Reaction Time and Movement,' Psychol. Monog. Sup., 1, 1904. Moore writes, "In one and the same series the reaction time undergoes considerable change, but the movement time is fairly constant. If you introduce factors which increase the difficulty of attention, the reaction time is lengthened and rendered still more variable, but the time of movement remains about the same," p. 58. "That the efficient path from cortex to muscle is not affected by the disturbance of the attention and that reaction time is not lengthened by any changed conditions along the path, seem to be conclusions warranted by the fact of constancy in the time of the movement by which the reaction was executed," p. 59.

[4] *Op. cit.*, 73–74.

excitation better calls out attention cannot be of very great importance. Concerning his final hypothesis, that the explanation of the relation between reaction time and intensity of stimulus lies chiefly in the latent period of the sense-organ, Piéron himself admits that it is an hypothesis, the truth of which he cannot demonstrate. One of the principal arguments which he suggests in favor of his conception is the fact that the data at hand indicate that the relation between intensity of stimulus and reaction time differs in the case of different senses.

In spite of the lack of evidence in support of the conclusions of Piéron concerning the rôle of the sensory latent period, they have a certain plausibility which would no doubt lead many to agree with him. Others, however, like Sherrington, would give more stress to the rôle of the synapse. Sherrington calls attention to the fact that the latent period of reflexes, that is, the time between application of stimulus and appearance of end-effect, increases with decrease in the intensity of the stimulus.[1] Thus, he finds that the latent time of the scratch-reflex varies from 140 σ with intense stimulation to 500 σ, or even several thousand σ, with weak stimulation. He writes: "This slackening of propagation speed under weak stimuli is, I would urge, a more significant difference between reflex-conduction and nerve-trunk conduction than is the mere greater slowness of the former than the latter."[2] This difference between conduction in reflex-arcs and nerve-trunks is referable to that part of the arc which lies in the gray matter. The slower conduction in the gray matter as well as the increase in reflex time with weak intensity of stimulus is supposed to be due to phenomena of transmission occuring at the synapse, where a surface of separation acts as a resistance or barrier to the passage of the nervous current from one neurone to the next.

The weaker stimulus, then, is to be thought of as resulting in a longer reflex time because it is delayed longer in transmission from one neurone to the next, than is the more intense

[1] "The Integrative Action of the Nervous System," 1906, 21.

[2] *Loc. cit.*

stimulus. Sherrington, apparently, does not regard the effect of intensity on reflex time as due in any appreciable degree to the latent period of the sense-organ.

Now, while the prolongation in reflex time with decrease in the intensity of the stimulus is a different phenomenon than the prolongation in human reaction time with decrease in intensity, yet it seems highly probable that the essentials of the explanation are the same in both cases. At any rate, there is just as much evidence that the effect of intensity upon reaction time is to be explained by reference to the impeding action of the synapse, as that the effect of intensity upon reflex time is to be so explained. Fully as many synapses are involved in the voluntary reactions of human beings as in the reflexes of the dog, and any effect which results from the nature of the synapse must be present in both cases and must be explained in both cases by the action of the synapse.

The inadequacy of both the sensory latent period and the synapse theories may be shown, to a large extent, by the same argument: for after all, both theories offer much the same explanation of the prolongation in reaction time with decrease in intensity of stimulus. In both cases, the concept of resistance to be overcome is the fundamental one; in one theory, the resistance is thought of as occurring at the first sensory neurone met with by the stimulus and in the other, as occurring at every synapse. It is the same concept that is constantly used in attempts at explaining the physiological side of all manner of psychological phenomena.[1] The excitation produced by the stimulus has to exceed some minimal limit, or threshold, before it is sufficient to bring about a reaction. A small force has to act longer to produce a given effect than a large force: consequently, it is argued, the weaker the stimulus, the longer the time to produce the required degree of excitation.

Now is such an explanation as the above tenable? We

[1] Ladd and Woodworth write as follows: "In fine, it seems possible to conceive the action of the nerve-centers as a process of the transmission of nerve-impulses that is subject to the peculiarities of central conduction. Most of these peculiarities can be stated in terms of resistance—resistance in general high, but variable with many conditions." 'Elements of Physiological Psychology,' 1911, 287.

must conclude that it is not, if for no other reason than that it does not hold for cessation reactions; for, if we argue that a large resistance increases the time required for any stimulus to produce its effect, we cannot also argue that this same large resistance also increases the time required for the disappearance or the cessation of the effect. On the contrary, the disappearance of the effect should be hastened. A few analogies will make this clear. If we make a slight impression on the skin, it will disappear, or fall below a prescribed threshold, quicker than if it is deep. The after-image of a bright light lasts longer than that of a faint light. A string vibrating with large amplitude requires a longer time to come to rest, or to fall below any given threshold, than one vibrating with small amplitude. The plasma membrane of a nerve cell would recover from a slight increase in permeability more quickly than from a great increase in permeability.[1] Such illustrations, which could be multiplied indefinitely, all show how improbable it is that any resistance offered by the first sensory neurone, or even by the synapse, would act in such a way that it would not only impede the action of a weak stimulus more than that of a strong, but would also impede the cessation of the action of the weak more than that of the strong.

One might object, and not without some justification, that this argument leaves out of consideration the fact that the resistance offered by the synapse may not be resistance to the rise or fall of the excitation but merely to its conduction. This resistance, it might be alleged, merely results in the weak current traveling along the nerve fibre more slowly than the strong. In all cases, the awareness of the cessation of the stimulus would be delayed by the time required for the very rearmost part of the stream of excitations produced by the stimulus to pass from the sense-organ to the cortex. Now,

[1] For an excellent discussion of the electric phenomena in nerve and muscle cells, see Höber, 'Physikalische Chemie der Zelle und der Gewöbe,' 4th ed., 1914, especially Chap. XII., 'Elektrische Vorgänge an physiologischen Membranen.' Cf. also Lillie, 'The Relation of Stimulation and Conduction in Irritable Tissues to Changes in Permeability of the Limiting Membranes,' *Amer. J. of Physiol.*, Vol. XXVIII., 1911, 197–222.

one might argue that this time would be greater for weak than for strong nervous excitations. Against this view, however, we have the results of recent physiological investigations, which show that the rate of transmission of nervous impulses is independent of their magnitude.[1] More decisive is the fact, apparently well established,[2] that the 'all-or-none' law applies to the normal nerve-fiber. This law is to the effect that any stimulus which excites a nerve-fiber at all will produce a maximal excitation. It follows that differences in intensity of excitation of sensory nerves is due to difference in the number of nerve-fibers (or conducting elements) stimulated, or else, as indicated by the results of Fröhlich,[3] to variation in the frequency of secondary excitation waves. Now, if the 'all-or-none' law be true, it is impossible for any number of synapses to delay the excitation produced by a weak stimulus more than that produced by a strong one. Consequently, the assumption of resistance offered by the synapse cannot account for the marked prolongation in either reaction time or reflex time with decrease in intensity of stimulus.

It is true that fatigued nerve-fibers do not follow the 'all-or-none' law. It might be held that, in a similar way, a sensory nerve-fiber subjected to stimuli such as light or sound would not follow this law. The high frequency of the secondary oscillations, in the case of the excitations produced in an optic nerve by a light stimulus, might result in the nerve taking on the properties of what Verworn calls an 'heterbolic' system,[4] and consequently in its showing variation in magnitude of excitation with variation in intensity of stimulus. However this may be, it is very improbable that such differences in intensity of excitation of single nerve-fibers (assuming them to exist) could account for the increase

[1] In confirmation, see the following: Gotch, *Journ. of Physiol.*, 1902, 395. Koike, *Ztsch. f. Biol.*, 1910, 310. Lucas, *Journ. of Physiol.*, 1911, 46. Adrian, *Ibid.*, 1912, 389; 1913, 384.

[2] Gotch, *Journ. of Physiol.*, 1902, 392. Symes and Veley, *Proc. Roy. Soc.*, B. lxxxiii., 1910, 431. Verworn, *Ztsch. f. allgm. Physiol.*, 1912, 277. Veszi, *Ztsch. f. allgm. Physiol.*, 1912, 321. Adrian, *Journ. of Physiol.*, 1913, 389. Lodholtz, *Ztsch. f. allgm. Physiol.*, 1913, 269. Lillie, *Amer. Journ. of Physiol.*, Vol. XXXIV., 1914, 410.

[3] *Ztsch. f. Sinnesphysiol.*, Vol. XLVIII., 1914, 28–165.

[4] *Ztsch. f. allgm. Physiol.*, 1912, 289.

in time of cessation reactions with decrease in intensity of stimulus. This becomes very evident from a consideration of reactions to a decrease in intensity, of which, after all, cessation reactions are merely a limiting case.

The assumption of greater slowness of conduction of weak excitations than of strong, even were it warranted, would not account for the fact[1] that a slight decrease in intensity of a strong stimulus results in a longer reaction time than a larger decrease in the same stimulus: for it cannot be held that a slight decrease in excitation is conducted more slowly than a large decrease. The decrease itself is not conducted at all, but merely the excitation as it exists before and after decrease. It may be urged that this argument is not pertinent, but I think there is little doubt that just as the reaction time to the beginning of a stimulus equals that to its cessation, so does the reaction time to an increase in intensity equal that to a decrease of the same size. We know, at least, that the reaction time, whether the reaction is to an increase or a decrease in intensity, is lengthened as the size of the change in intensity is decreased. Reactions to a decrease in intensity should, then, be considered along with cessation reactions.

Another point to be considered, in addition to the effect of intensity of stimulus upon reaction time, is the difference in reaction time between sound and light. Reactions to a moderately bright light are longer than those to a moderately loud sound. This fact is not uncommonly explained by assuming that the latent period of stimulation of the sense-organ is longer in the case of light.[2] Against this assumption, we may urge the following considerations:

First, since cessation reaction times are the same as beginning reaction times in both sound and light, we would have to assume that in both cases the latent period for the

[1] See Woodrow, 'The Measurement of Attention,' Psychol. Monog., 1915, Chap. IV.

[2] Ladd and Woodworth write as follows: ". . . there seems good reason to suppose that the reaction time of sight is necessarily longer than that of hearing or touch, on account of the photochemical nature of its more immediate stimulus." "On the whole, the suggestion which probably is most generally entertained . . . is that already adopted by us,—namely, that the inertia or latent time of different sense-organs differs." 'Elements of Physiological Psychology,' 1911, 472.

dying out of the excitation just equalled that for its rise, that is, not only assume that the photochemical process was longer in getting started but also equally longer in dying down. Second, if we assumed any considerable latent period of stimulation, it would seem reasonable to suppose that such latent period would increase markedly with weakened intensity. But as has been pointed out in preceding sections, such an increase in latent period is incompatible with the fact that reactions to the cessation of a very weak stimulus are longer than to the cessation of a strong one. At any rate, the effect of intensity, which cannot be explained by reference to the latent period, is sufficient to completely overwhelm the effect of the latent period, since a very weak sound may give much longer reaction times than a very bright light. Third, in the case of reflexes, we know that different reflexes may vary greatly in time even when they are elicited by direct stimulation of the sensory nerve, so that the latent period of stimulation of the sense-organ is not involved. Similarly, a great variation in the time of different reflexes occurs where the sense-organ stimulated remains the same. For example, the latent time of the scratch-reflex is, on the average, very much longer than that of the flexion-reflex of the same limb, although the distance of nerve fiber conduction is not greater.[1] Fourth, and lastly, it can be shown, as will be pointed out below, that the degree of attention is less with light reactions than with sound, and that this fact offers an intelligible explanation of the variation in reaction time with mode of stimulus. It is true that reflexes as a rule do not involve attention, but since the work of Sherrington, showing the important rôle of the central nervous system in determining the characteristics of reflexes, it may very well be assumed that a nervous mechanism analogous to the nervous mechanism of attention, though less complex and without conscious accompaniments, is involved in every reflex.[2]

One other point may be mentioned as one which should be

[1] Sherrington, *op. cit.*, p. 21.

[2] "The interference of unlike reflexes and the alliance of like reflexes in their action upon their common paths seems to lie at the very root of the great psychical process of 'attention.'" Sherrington, *op. cit.*, 234.

illuminated by knowledge of cessation reactions. I refer to the long time required by a weak intensity of stimulus to produce its maximal intensity of sensation. The data here presented do not deal exactly with this point, but the experiment with the liminal sound as stimulus is of interest in this connection. It is known that a sound of constant physical intensity increases in apparent intensity up to a duration, in the case of weak sounds, of 1.5 secs.[1] It ought, then, to be possible, by using a sound so weak as to be inaudible until nearly its maximum effect is produced, to obtain a sound so weak that it could not be heard at all for a large fraction of a second. The reaction time I actually obtained for such a sound was about three fourths of a second,[2] and the reaction time to its cessation was fully as long. In the case of a liminal sound such as here used, there can be no doubt that the reaction does not occur until the subject is aware, in the case of beginning reactions, of the presence of the sound, and in the case of cessation reactions, of its absence. It is well known, apart from the present data, that it takes longer to become aware of a weak stimulus than of a strong.[3] It may be said, then, that the very long reaction time in the case of beginning reactions shows how very long it takes to become aware of a liminal stimulus. Moreover, in view of the approximate equality of the beginning and cessation reaction times, it may be concluded that just as it takes longer to become aware of a weak stimulus than of a strong, so does it take longer, and about equally longer, to become aware of the cessation of a weak stimulus than of a strong.

Here again, we may raise the question of explanation by resistance. And there can be little doubt that the explanation of the period required for the rise of the excitation up to the awareness threshold is closely connected, if not essentially

[1] Kafka, 'Ueber das Ansteigen der Tonerregung,' *Psychol. Stud.*, 1907, 256–292. See also Sander, 'Das Ansteigen der Schallerregung bei Tönen verschiedener Höhe,' *Psychol. Stud.*, 1910, 1–38.

[2] The great discrepancy between this value and the corresponding values obtained by Wundt (337 σ) and by Piéron (361 σ) is probably due to the stricter insistence upon liminal intensity of stimulus in the present instance.

[3] See Minneman, 'Untersuchungen über die Differenz der Wahrnehmungsgeschwindigkeit von Licht und Schallreizen,' *Psychol. Stud.*, 1911, 1–82.

identical, with the explanation of the period required for the further rise to a maximum. In the explanation by resistance, in the present instance, the resistance would probably be thought of as at the synapse; but no mode of behavior on the part of the synapse has yet been suggested which would explain how it can offer resistance both to the rise and to the fall of the excitatory process. The cessation of the excitation is not a new excitation as we know from the introspection that the sound sensation merely ceases,—no new sensation is set up, when the stimulus is cut off. So it is evident that neither the long period required for the rise of the excitation up to the sensation or awareness threshold, nor, in all probability, the further rise to a maximum, can be explained by reference to the concept of a surface of separation at the synapse or to any other form of resistance to conduction along nervous arcs.

A consideration of the facts of cessation reactions, then, leads to certain negative conclusions concerning the explanatory value of the concept of resistance, whether this resistance is placed at the point of stimulation, at the synapse, or in the whole neurone. Such resistance cannot explain the fact that cessation reactions to a weak stimulus are longer than to a strong one, but on the other hand would tend to cause us to expect the reverse. Moreover, since both cessation and beginning reactions follow the same law as regards variation in time with variation in intensity of stimulus, we should expect the same explanation in both cases; and so, if resistance to conduction does not explain the one, it probably does not explain the other. Again, as regards the difference in reaction time between sound and light, this, likewise, does not seem explicable by reference to the greater resistance of the retina to stimulation. Further, since it takes as long to become aware of the cessation as of the beginning of a very weak sound, the long time required for the awareness of either cannot be due to resistance to conduction. And, moreover, this latter fact renders it probable that such resistance has nothing to do with the explanation of the long time required for a weak stimulus to produce its maximal sensory effect.

While the facts of cessation reactions thus serve to call

attention to the shortcomings in the explanation of the behavior of the nervous system by means of the concept of resistance, it must be admitted that there exists a large accumulation of other facts which likewise greatly impair the plausibility of such explanation. Among this mass of evidence, we may cite the great disproportion and lack of correlation between energy of stimulus and energy of response. Even more conclusive is the temporal discrepancy. This discrepancy is especially striking in cases where psychocerebral processes are involved, but it is very evident even in reflexes, in the phenomenon of the after-discharge. Concerning this phenomenon, Sherrington writes as follows: "Tetanic contraction of the knee-flexor muscles of the dog induced by brief faradization of the motor-nerve usually ceases within 150 σ of the cessation of the stimulation of the nerve, if crude condition of fatigue, etc., be avoided. The contraction of these same muscles, when induced reflexly by a similar brief stimulation, often persists for 5,000 σ after cessation of the stimulus.[1] Now this great duration of the after-discharge certainly cannot be explained by resistance offered at the synapse; and it is equally certain, that resistance at the synapse cannot explain the relation between intensity of stimulus and *both* duration of after-discharge and reflex latent time, since the duration of the after-discharge *increases* with intensity of stimulus, while the length of reflex time *decreases*. It would be easy to continue, indefinitely, these instances of the unsatisfactory nature of the theory that the action of the nervous system is explicable merely by the assumption of a network of reflex paths, which offers more or less resistance to the passage of currents from the sense-organs to the muscle. However, the following two citations will suffice to indicate the dissatisfaction with such a theory.

Titchener writes as follows: "The assumption that the reflex arc is the unit of nerve function evidently makes the brain nothing more, in principle, than a mass of superposed reflex arcs; the central is assimilated to the peripheral mechanism; the office of the brain is to perceive, to couple up, and

[1] *Op. cit.*, 26.

to send out. But this view that the nervous system is a system of conduction, a sort of glorified telephone exchange, is in the author's opinion wholly inadequate to explain the phenomena of mind. The theory of conduction, with obstacles or easements between cell and cell, must, he believes, be replaced by a theory of intracellular change, or change within the cell-body; and if this is the case, the cortex must be regarded rather as a disjunction of the reflex arc than as a switchboard for the manifold connection of afferent with efferent process."[1]

Reference may be made also to the views of the physiologist, T. Graham Brown. From experiments which demonstrated that the phenomenon of "narcosis progression" in the cat may occur at a depth of narcosis at which the spinal reflexes are abolished, and from a general consideration of the facts of rhythmic motor phenomena, Brown concludes: "The fundamental unit of activity in the nervous system is not that which we term the spinal reflex." He says that his experiments "show the independence of the efferent neurone, and suggest that the functional unit is the activity of the independent efferent neurone; or rather, that it is the mutually conditioned activity of the linked antagonistic efferent neurones ('half-centers') which together form the 'center,' and they also suggest that the primitive activity of the nervous system is seen in such rhythmic acts as progression and respiration."[2]

We may now take up the specific and difficult question of how the results obtained with cessation reactions, discussed above, are to be explained. We have seen that the concept of resistance is inadequate. Is it possible to offer a more satisfactory explanation? While in the present state of nerve physiology it would require an unprofitable degree of speculation to attempt a detailed explanation, it does seem feasible to indicate at least the general direction which such an explanation must take.

[1] 'A Text-Book of Psychology,' 1911, 489.

[2] T. Graham Brown, 'On the Nature of the Fundamental Activity of the Nervous Centers, Together with an Analysis of the Conditioning of Rhythmic Activity in Progression, and a Theory of the Evolution of Function in the Nervous System,' *Journ. of Physiol.*, Vol. XVIII., 1914.

In the first place, it seems clear that a greater and more distinctive rôle must be given to the central nervous system, a rôle such that the activity of the central nervous system cannot be said to be assimilated to the peripheral. To substantiate this proposition I shall refer to some results I have already published elsewhere, on the effect of the preparatory interval on reaction time.[1] These results show that a much longer reaction time is obtained with a set of totally irregular preparatory intervals of varying length, or with very long preparatory intervals, than with a regularly repeated preparatory interval of two seconds. We may speak, then, of the prolongation in reaction time produced by the unfavorable preparatory intervals. Now I have shown that with weak intensity of stimulus this prolongation is very much greater than with strong or moderate intensity; and I have pointed out at length,[2] that this result can be explained only on the assumption that in the case of the weaker intensity we are dealing with a lower degree of attention. Moreover, it is known that with weakened attention we have lengthened reaction time. We have, then, experimental proof that a large part, at least, of the increase in reaction time with decrease in intensity is due to a decrease in the degree of attention. This conclusion is in general agreement with the explanation long since advanced by Wundt[3] and by Martius.[4] The basis of the explanation is the fact that intensity of stimulus is a condition determining the degree of attention; and I shall attempt to show how, on this basis, we can explain the laws of cessation reactions as well as those of ordinary reactions.

Another investigation which I desire to mention, is one on the measurement of attention as it occurs in reactions to sound, light, and touch, using as the measure of attention the reciprocal of the prolongation in reaction time produced by unfavorable preparatory intervals. While these results may not be published for some months yet, it is perhaps permissible

[1] 'The Measurement of Attention,' Psychol. Monog., 1915.

[2] *Op. cit.*, Chap. III.

[3] 'Physiologische Psychologie,' 5th ed., 1903, Vol. III., p. 430.

[4] 'Ueber den Einfluss der Intensität der Reize auf die Reactionszeit der Klänge,' *Philos. Stud.*, 1892, 469–486.

to state that, in the case of nearly all of the twelve subjects so far measured, with whom over 15,000 reaction times have been taken, the greatest prolongation occurred with light, the next greatest with sound, and the smallest with touch; from which I conclude that the degree of attention is in general lowest in the case of light reactions, and highest in the case of touch reactions. I am convinced from an analysis of the data, that the difference in degree of attention in these cases is sufficient to account for the difference in simple reaction time that occurs with difference in mode of stimulus.

Befor making use of these results in the explanation of the phenomena here under discussion, I wish to emphasize the fact that the reactions in human reaction time experiments are not at all simple reflexes. Of course, cessation reactions can never be what we ordinarily think of as reflexes, for in this case there is no stimulus, merely the cessation of a stimulus. One cannot mince words in such a fashion as to say that the cessation of stimulation is stimulation. The reflex theory would seem entirely inadequate to account even for the existence of cessation reactions. A similar point could be made in the case of reactions to a mere *decrease* in intensity. Moreover, the reaction is only in a very secondary manner a reaction to the change in stimulus. It is primarily brought about by the subject's intention to react, which in turn is the result of acceptance of instructions given the subject a considerable time before the reactions. It is further evident, at least in the case of weak intensities, that the reaction does not occur until the subject becomes aware of the change in stimulus. The process of inhibition, so far as can be judged either from introspection or from the objective reaction times, is present equally in cessation and beginning regctions. In both cases the subject is prepared to react when the right time comes, but this preparation remains merely a preparation until then. In one case the 'right time' means when the subject becomes aware of the stimulus, and in the other case, when he becomes aware that the stimulus has ceased. No satisfactory account of the reaction process can be given without taking into consideration this intention and preparation to react.

In the present state of our knowledge of the physiology of the nervous system, it is impossible to say definitely in what the preparation to react consists. We know that, in practiced subjects, it is often unaccompanied by any appreciable tension on the part of the muscles of the fingers used in the reaction. It appears, therefore, not to consist in any actual innervation of the specific reaction movements. We know, too, that this preparation is constantly undergoing certain irregular variations in degree, that it takes about two seconds to reach its maximum, and that thereafter, in addition to irregular oscillations, it shows a definite general tendency to weaken. It should be emphasized, however, that a certain degree of adaptation may be maintained throughout a considerable period of time. We know, further, that this preparation can vary in degree or intensity, and that the greater the intensity of such preparation, the quicker the reaction. Without attempting to go into details, it is evident that the preparation to react means, on the nervous side, the presence of certain nervous energies which are held in check, so far as a reaction movement is concerned.

There is energy, the intimate nature of which we must admit is as yet unknown, present in the central nervous system and temporarily inhibited in some way from producing the reaction. It is probable that this energy is of an electrical nature, and that it always involves many, if not all, of the neurones of the central nervous system. The condition within any one neurone is to be thought of as interrelated with the conditions within all the others, so that there is always involved an immensely complicated and widespread system of energies, including, perhaps, both potential chemical energies and electrical activities. So long as the subject's state is merely one of *preparation* for the reaction, it must be supposed that the interrelated cortical energies, while by no means entirely suppressing each other (for the subject would then be unconscious), yet, interact in such a way that some sort of equilibrium occurs at the point of exit of the motor pathway for the reaction movement. The existence of this equilibrium, as respects the reaction movement, is evident

merely because the reaction movement does not occur, ordinarily, until after the stimulus which comes at the end of the preparatory interval.

The mere fact that both beginning and cessation reactions are possible, shows that this equilibrium may be upset either by the beginning of some new disturbance, or by the cessation of one. There is no theoretical difficulty here such as we encountered in attempting to explain both the laws of cessation and beginning reactions by reference to the concept of resistance. Different instructions set up different central adjustments, and while these adjustments in both cases consist in a system of activities which are in equilibrium so far as the reaction movement is concerned, the pattern of the adjustment must be different in the two cases; so that, in one case, the occurrence of a new excitation destroys the existing equilibrium, while, in the other, the new excitation merely serves to increase the intensity of activity of a different system, the equilibrium of which, however, is not upset until this excitation ceases. The widespread system of cortical energies takes on such a form, as the result of the instructions, that upon the occurrence of the proper sensory disturbance, some of this cerebral energy is released for the work of innervation of the reaction movement.

The energy used in the innervation of the reaction movement is not the energy of the excitation set up in the sensory nerve by the stimulus; it is energy already present in the central nervous system before the stimulus acts. In a reflex, as commonly described, the outgoing energy is merely the redirected incoming energy. This remains true, even though we further add that reflexes may inhibit or reinforce one another. The present view, however, regards the central nervous system as not merely a network of paths, but also as a vast system of interrelated energies, potential and otherwise. A disturbance produced in this system is not conducted through it; it causes a readjustment of the system, which readjustment may (or may not, so far as is known) result in the release of energy for the work of motor innervation. It is difficult to see how, on any other theory, we

can account even for the fact of cessation reactions. It has already been pointed out that it is impossible to think of the cessation reaction as a simple reflex, for in this case there is no stimulus, but merely the cessation of a stimulus. On the other hand it is not difficult to understand that a change in any part of a system of energies may upset the balance of the system as a whole, whether the change in question be an increase or decrease of activity.

The process by which the change in sensory excitation disturbs the preëxisting cerebral system is in part, no doubt, the physiological process corresponding to becoming aware of the change. Even if actual awareness were not a necessary condition of the reaction, it seems altogether probable that a nervous process identical in nature to that which underlies awareness, though less in degree, must precede the reaction movement. The process which underlies awareness of the cessation of a stimulus must consist in an effect produced by this sensory change upon more or less widely distributed parts of the cerebral cortex. It could not consist in the local sensory effect, for this is merely a cessation. It is probably no less true that the awareness of the occurrence of the stimulus involves changes in the entire cerebral activity. We know from introspection that any new perception coming into the field of consciousness ordinarily modifies the pattern of consciousness as a whole.

It is clear that these central nervous processes in which alone is to be found the explanation of the phenomena discussed in the preceding pages, are in large part identical with those of attention or awareness; but that these central processes are all such that they may be subsumed under this heading is improbable. They are quite likely very complex. It may be, for instance, that in addition to the physiological process underlying awareness, there occurs another process which consists in the release of so-called determining tendencies.

We may now ask why a small change in the sensory excitation, whatever this may mean in terms of nerve physiology, should bring about the required disturbance in the pre-

existing system of cerebral energies so much more slowly than a large one, regardless of the direction of change. This is satisfactorily explained as due to the inertia of the pre-existing cortical adjustment. Of course we do not know the intimate nature of the cortical changes brought about by the sensory change; but that there is work to be done in bringing about these charges, and that this work would be done more slowly by a small local disturbance than by a big one, seem to me unquestionable propositions. I have already argued that the cortical processes in question are largely those underlying the phenomenon of attention. It is well known that these processes require considerable time; *e. g.*, it is well established that maximal adaptation of attention requires about two seconds. There is, moreover, no difficulty in understanding why the reaction time to the cessation of the excitation equals the reaction time to its occurrence, no matter what the intensity of the stimulus. The size of change in sensory excitation is of course the same, whether the change be due to the occurrence or the cessation of the stimulus. The direction of change of the excitation need not affect either the size or rate of change. The rate of change in the sensory cortical excitation, then, we assume to be practically the same at both the occurrence and cessation of the excitation; and, further, we assume that so long as the intensity of the preparatory adjustment remains constant, the time required for the disturbance or release of the preadjusted system of cortical energies varies with the size and rate of change in excitation, which brings about this release. The difference in direction of the change in excitation is counterbalanced by the difference in the nature of the preadjustment.

The next question to be answered is why light reactions, at moderate intensities of stimuli, are longer than sound reactions. This is easily accounted for on the hypothesis that the cortical preadjustment is less effective in the case of light than in the case of sound. The lesser effectiveness of preadjustment, in the case of light reactions as compared with sound or touch reactions, may possibly be accounted for by the fact that the adjustment of the sense-organ in the case of

vision—including the control of convergence, accommodation, winking, proper direction of head, etc.—is more elaborate than in the case of hearing or touch, where no sensory adjustments of any great consequence are required. The expenditure of energy on the sensory adjustment may detract from the adequacy of the central cerebral adjustment as a preparation for the reaction movement. Whatever be the cause of the better cerebral adjustment in the case of sound and touch reactions, the fact of such better adjustment is evident; for upon no other assumption can we explain the result already mentioned, that light reactions are prolonged more by the use of unfavorable preparatory intervals than are sound or touch reactions. I have shown elsewhere, that the prolongation produced in reaction time by unfavorable preparatory intervals is due, solely, to the effect of such as a detractor of attention. This detraction effect occurs in accordance with the law that the prolongation produced varies inversely with the degree of attention acted upon. Accordingly, since the prolongation in reaction time produced by unfavorable preparatory intervals is greater in the case of light reactions than in touch or sound, it follows that the degree of attention in the former is less than in the latter. On the cerebral side we may substitute effectiveness of cerebral adjustment for degree of adaptation of attention.

Conclusions

1. The experiments here reported show that the reaction time to the cessation of a sound or light stimulus is in all cases the same as the reaction time to the beginning of that stimulus. This statement includes the following two corollaries. (1) The same difference between reaction times to sound and light exists in the case of reactions to their cessation as in the case of reactions to their beginning. (2) The lengthening in reaction time due to a decrease in intensity of stimulus is equal in amount, and follows the same law, in the case of both beginning and cessation reactions.

2. The above mentioned similarity, in the variation of both beginning and cessation reactions with mode and with

intensity, cannot be explained either by the hypothesis of variation in the latent period of stimulation of the sense-organ, or by the hypothesis of variation in the resistance offered by the synapses. Likewise, this similarity cannot be adequately explained on the commonly accepted hypothesis that all nervous action consists essentially in conduction of nervous impulses from the sense-organ to the muscle along arcs which offer varying amounts of resistance. On the other hand, the facts on beginning and cessation reactions are incompatible with such an hypothesis.

3. The explanation of the experimental data presented in the preceding pages, while at present impossible in detail, seems to require us to regard the central nervous system as not merely a network of paths, but also as the seat of a complex system of interrelated activities and potential energies which is disturbed throughout by any change in any part of the system. The fact of cessation reactions cannot be adequately explained without postulating such a central system of energies, the balance of which may be upset by either an increase or decrease of activity in any part of the system. The pattern of the preëxisting system differs in beginning reactions from that in cessation reactions in such a manner that, in beginning reactions, the same effect is produced by the rise of the excitation as is produced in cessation reactions by its fall. A small change in excitation disturbs the preëxisting cortical system so as to bring about the reaction movement more slowly than does a large one—not because of resistance to its conduction, but because of the inertia of the preëxisting central system. The reaction time to light, both for cessation and beginning reactions, is longer than for sound because of an inferior preadjustment of the cerebral mechanism. In all cases the reaction results from the release of energy already within the nervous system before the occurrence of the stimulus; and is not due to the mere redirection or modification of the incoming sensory excitation.

4. The physiological disturbance of the central nervous system here involved is in large part, though not entirely, that which underlies the process of becoming aware of a

stimulus or of attending to it. This is shown by the fact that the degree of attention, in the case of reactions to a weak stimulus, is less than that in the case of reactions to a strong stimulus, and also, less in the case of reactions to light than in the case of reactions to sound or touch.

FACILITATION AND INHIBITION OF MOTOR IMPULSES

A Study in Simultaneous and Alternating Finger Movements

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Description of Experimentation

The purpose of the investigation[1] was to ascertain the facilitating and the inhibitory effect of successive and simultaneous muscular impulses in the movements of the several fingers of both the right and left hands. The data have also been arranged to show the effect of practise and fatigue and the relation of the movements of the fingers of the right hand to those of the left hand.

The instrument used was similar to that of the Whipple tapping-board[2] as will be seen from the cut, p. 476. In place of a metal stylus, a ring was worn on the finger. This was insulated by rubber tubing except on the under part where a metal peg protruded and made an electric contact with the metal base of the board. A light flexible wire ran from the ring to an electric marker which registered the contact on a kymograph.[3] In order to obtain a free and natural movement of the fingers, the hand rested on a curved block of wood, and was thus slightly vaulted. The tips of the fingers and cushions of the palm of the hand rested on the board. The arm rested on the table, several of the subjects using a cushion for greater comfort.

It was deemed of importance that the height of the finger

[1] The experiments were conducted in the Harvard Psychological Laboratory during the academic year, 1913–14. Trial experiments, not here reported, were made the previous year.

[2] 'Manual of Mental and Physical Tests, Simpler Processes,' p. 131.

[3] It was under similar conditions that one of the first tapping tests—that of von Kries—was performed, 'Zur Kenntniss der willkürlicher Muskelthätigkeit,' *Arch. f. Anatomie u. Physiologie*, 1886, Sup. Band, pp. 1–16.

movement should be constant so that there could be no doubt that differences in the rate of tapping were not due to changes in the length of stroke. A bar was therefore placed 4 cm. above the board and the subject was instructed to hit the bar at each stroke. The bar met the finger slightly below the second joint, and care was taken that it should always strike at approximately the same spot throughout the tests. This arrangement worked very well, the subject soon becoming used to the task. The movement thus remained more nearly voluntary than would probably have been the case if the finger had not had to touch the bar.[1] The hand was not strapped to the board as it was soon evident that the subject could keep his hand and arm still, and if there did happen to be a slight movement, his attention was called to it at once; nor was the tapping continued to that period of fatigue when the subject in seeking relief begins to use other muscle groups of the arm. Any slight error that might have crept in could not be as great as would have been the danger to the result caused by the binding of the muscles. The only muscles used, therefore, were the extensors and the flexors of the phalanges. These muscles pass from their origin in the forearm over the wrist joint to the phalanges and at the end of the grasping movement there is a tendency for them to flex the hand, which is inhibited by the synergic muscle of the wrist.

The time in two-second periods was marked on the drum at the beginning of each set of trials by a marker actuated

[1] In regard to the length of stroke, von Kries writes, *loc. cit.*, page 4, "Der Umfang der Bewegungen ist auf die Dauer von nur geringem Einfluss; doch scheint es, dass die Bewegungen von einem gewissen mittleren Umfamg am schnellsten ausgeführt werden können und sowohl die sehr kleinen als die sehr grossen ein wening länger dauern." Bryan ('On the Development of Voluntary Motor Ability,' *Amer. Jour. of Psychol.*, 5, 1892, p. 150) is in agreement with von Kries as to the slight effect of change of amplitude. Max Isserlin states that ". . . die Tendenz besteht, trotz abnehmender Geschwindigkeit die Bewegungszahl konstant zu erhalten. Diese wird zuletzt herabgesetzt" ('Ueber den Ablauf einfacher willkürlicher Bewegungen,' *Psych. Arbeiten*, 6 1910, p. 186). From this we may conclude that the change in amplitude conceals the fatigue as measured by the rate of tapping alone. It was also found in the preliminary tests before the control bar was used that there was a strong tendency for several subjects to execute a series of quick reflex movements similar to a tremble which greatly increased the tapping rate and seemed difficult at times to avoid.

by the laboratory clock. When both hands were used, a board was employed for each hand.

There were four subjects who will be referred to as *A*, *B*, *C*, *D*. *A* and *B* were advanced graduate students; *C* and *D* undergraduates. *A* and *D* were very athletic, *B* less so and *C* the least strong of the four. The experiments were made in the morning, the subjects coming always at the same hour. As a rule, there was a week's intermission between each set of trials. The period of tapping for all fingers and all combinations of movements was 30 seconds with a two seconds' pause between the members of a series and a five minutes' pause between the series. The finger movements examined were as follows: During the first half year, the subject began by tapping with his right index finger (*R*1). This was followed by the second finger of the same hand (*R*2). Then these two fingers were tapped alternately (*A*), a movement similar to the walking movement, that is, *R*1 was raised as *R*2 was lowered, the two fingers passing each other in the middle of the stroke. There then followed what may be termed complete alternation (*CA*). *R*1 made a complete stroke up and down before *R*2 began. The signal for a finger to begin was the return of the other finger to the starting point in the manner of a relay race. *R*1 went up and down then *R*2 went up and down, etc. Finally *R*1 and *R*2 tapped simultaneously [*S*(*R*1 *R*2)]. This completed the series. After a five minutes' pause, the series was repeated in the same order. On alternate weeks, the order of the series was reversed, beginning with *S* and ending with *R*1. At the beginning of the year the left-hand fingers were tapped in the same manner as the right and again for one series about six weeks later, in order to ascertain if there was any transfer of practise. During the second half of the year, both hands were used. The series began with the right index finger alone. Then followed the left index finger alone (*L*1) and then both simultaneously [*S*(*R*1 *L*1)]; and then the second finger of the right hand (*R*2) alone for thirty seconds, followed by the second finger of the left hand (*L*2) alone and then both simultaneously [*S*(*R*2 *L*2)]. This series was repeated and

the order reversed on alternate weeks. A few series were also made with *R*1 and *L*2 and [*S*(*R*1 *L*2)] and *R*1 and *L*4 and [*S* (*R*1 *L*4)].

Simultaneous Movement

The final averages have been gathered of the simultaneous tapping of the pairs of fingers of both hands and have been placed together in Table IV.[1] for convenience of comparison. In the second and fourth columns for each subject are the rates of tappings for 30″ for the fingers separately, and in the sixth column, the rates when tapped together. In the seventh column is the difference in rate of the two fingers, and in the eighth, the difference between the rate of the simultaneous tapping and the rate of the slower finger when tapping alone. Examining first the two fingers of the same hand, *R*1 and *R*2, we find the interesting fact that with all the subjects the two fingers are moved more rapidly together than the slower finger alone and in the case of *A* faster even than the faster finger by a considerable amount. With *B*, *S* is one stroke faster than the faster finger. A similar relation holds with the symmetrical fingers of the left hand (second horizontal column). Here in fact in the case of all but *B*, who shows little change, the two fingers are tapped faster together than either of the separate fingers. In Tables I., II. and III. the maximal rates of tapping are in heavy type and may be readily compared. For the right hand the highest *S* is greater than the highest *R*1 or *R*2 for all subjects but *C*. For the left hand the highest *S* is greater than the highest *L*1 or *L*2 for all the subjects. The explanation which suggests itself is that the two fingers being very closely related, an impulse to one tends to influence the other, since a strong coördination has probably been induced by the grasping movement. In the single movement the other finger must be voluntarily held down and this slows up the action of the moving finger. When both fingers are moved together this inhibition is removed and they both move faster, unless one is much slower than the other, when it acts as an inhibition. It is possible

[1] These averages have been taken from Tables I., II. and III.

that with the left hand, the inhibition of the idle finger is more difficult than with the right hand due to less practise. This would account for *S* being greater than the single tapping

TABLE I

No.	Subject *A*					Subject *B*					Subject *C*					Subject *D*				
	*R*1	*R*2	*A*	*CA*	*S*	*R*1	*R*2	*A*	*CA*	*S*	*R*1	*R*2	*A*	*CA*	*S*	*R*1	*R*2	*A*	*CA*	*S*
1	154	165	120	37	172		137	116	36	163	178	148	44	23	160	139	161	106	25	172
2	155	170	121	39	192	168	141	131	34	170	161	162		24	174	149	158	112	34	174
3	153	165	117	34	187	143	138	131	41	161	119	121	68	27	114	137	152	108	32	151
4	184	190	126	40	196	172	161	136	40	155	115	117		31	132	147	162	112	36	157
5	145	153	118	40	178	173	152	140	35	170	174	162	57	29	175	141	148	104	34	154
6	146	163	115	46	187	170	164	143	42	188	193	151	67	27	190	136	146	102	36	138
7	159	171	119	43	167	178	160	133	39	166	191	168	56	26	168	133	152	109	35	135
8	170	166	136	47	197	166	170	141	42	164	194	165	66	26	174	136	153	103	40	134
9	137	145	120	43	163	160	151	127	40	181	178	161	54	24	167	121	146	103	35	148
10	154	143	130	44	179	189	171	168	59		187	159	59	25	186		145	105	36	148
11	155	159	131	37	150	178	178	133	37	173						142	168	113	40	146
12	160	150	137	40	168	192	186	154	45	181						141	160	109	40	146
13	141	136	139	41	176	169	161	142	33	169						145	156	105	42	148
14	159	150	137	46	169	170	170	138	40	177						129	158	114	34	148
15	157	161	133	45	161	178	178	155	44	188						129	151	106	36	138
16	154	150	134	52	182	180	175	153	53	178						135	148	101	32	149
17						186	177	151	41	187						139	162	103	38	158
18						185	185	144	40	198						143	153	110	35	155
Av. R1 to S	155	158	127	42	176	173	164	141	41	174	169	151	59	26	164	138	154	107	36	150
S to R1	149	153	125	42	177	174	160	140	40	178	178	157	56	25	175	135	153	106	35	154
m.v.	161	164	129	42	176	173	168	142	42	170	159	142	63	27	147	138	156	108	36	144
	7	10	8	4	11	9	13	9	4	9	22	14	6	2	17	6	5	3	3	8

of either finger for the two subjects who did not show this with the right hand.

Is, however, the release of inhibition due to the close relationship of the fingers the only factor which causes the

TABLE II

No.	Subject *A*					Subject *B*					Subject *C*					Subject *D*				
	*L*1	*L*2	*A*	*CA*	*S*	*L*1	*L*2	*A*	*CA*	*S*	*L*1	*L*2	*A*	*CA*	*S*	*L*1	*L*2	*A*	*CA*	*S*
1	110	128	46	27	135	134	131	111	32	131	115	113	44	24	116	134	120	90	30	156
2	125	140	55	36	164	139	143	119	35	140	111	117	58	27	128	137	132	105	34	143
3	107	128	56	30	133	143	142	119	34	131	114	117	81	22	125	137	135	104	38	127
4	113	125	68	37	141	154	146	114	36	142	113	113	71	25	120	151	139	106	35	141
5	107	122	59	32	133	149	148	115	44	155	110	109	53	28	118	137	125	97	34	134
6	113	119	68	36	140	150	149	113	49	164	110	120	59	28	121	136	133	98	38	142
Av.	112	127	59	33	141	145	143	115	38	144	112	115	61	26	121	139	131	100	35	140
m.v.	5	5	6	3	7	6	4	2	5	10	2	3	10	2	3	4	5	5	2	7

increase especially in the slower finger? Does not one impulse directly influence the other when discharged simultaneously, not only exciting an inhibitory effect in the case of the slower movement, but a facilitating effect in the case of the faster? To answer this question symmetrical fingers of the two hands, *R*1 and *L*1 and *R*2 and *L*2 were tapped simultaneously. Here there cannot be the same strong natural tendency to move the two fingers simultaneously as is prob-

TABLE III

No.	Subject *A*						Subject *B*						Subject *C*						Subject *D*					
	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2	*S*	*R*1	*L*1	*S*	*R*2	*L*2	*S*
1	146	117	137	147	124	155	172	146	155	170	143	163	179	107	118	168	114	133	130	124	128	151	128	127
2	157	119	150	151	123	164	184	153	167	177	151	174	191	114	127	167	116	122	135	125	128	151	132	126
3	164	128	140	145	117	156	173	144	163	174	148	151	190	113	122	171	124	139	131	128	127	152	130	139
4	159	115	149	168	140	158	193	146	169	180	148	154	200	116	123	180	114	126	147	140	133	164	134	129
5	135	129	147	156	131	151	174	150	165	177	148	165	173	123	131	169	117	135	133	132	131	153	135	129
6	160	131	146	173	141	152	180	158	162	174	154	161	192	111	124	184	124	141	138	136	130	158	140	134
7	169	117	152	170	131	155	192	159	175	185	154	159	180	115	123	169	114	120	142	132	132	153	127	130
8	177	130	144	160	132	150	203	153	167	186	160	170	172	116	123	120	101	134	142	144	136	153	135	143
9	167	133	163	163	125	150	180	151	157	186	149	156	182	123	134	169	125	134	138	132	128	157	132	132
10	162	138	160	158	132	166	187	148	157	172	146	153	204	119	122	162	111	118	138	129	130	153	131	146
11	158	119	143	167	123	151	193	153	170	180	148	162	176	121	120	156	118	124	135	140	133	154	131	135
12	162	125	150	174	131	155	200	148	178	185	152	182	179	124	132	164	135	133	143	133	141	163	137	136
13							175	145	162	173	152	165												
14							186	152	162	172	158	160												
Av.	160	125	143	161	129	155	185	150	164	178	151	162	185	117	125	165	118	130	138	133	131	155	133	133
R1 to S	155	129	140	158	131	156	179	150	161	175	150	162	187	116	126	170	118	130	135	129	129	154	133	132
S to R1	166	122	146	164	129	154	192	150	170	180	152	163	181	118	124	160	118	130	140	136	134	156	132	135
m.v.	7	6	7	8	5	4	8	4	5	5	4	6	9	4	4	10	6	7	4	5	3	3	4	5

ably the case with the fingers just examined. In Table IV. we find that with three of the subjects for both sets of fingers there is an increase in the tapping of the slow and a decrease in that of the fast finger. In the case of both *A* and *B* the simultaneous tapping approaches the average of the two fingers. For *C* the effect of the faster finger is not so great and the increase of speed of the slower finger is below that of *A* and *B*. A further peculiarity of this subject to be discussed later offers an explanation for this. For *D* the simultaneous tapping for both pairs is about the same as the slower finger. The difference between *R*1 and *L*1, however, is very slight

and between *R2* and *L2* less than with any of the other subjects. This difference is an important factor as will be seen below (p. 460). It is evident, however, that with three of the subjects the rapidity of the movement is increased by the simultaneous exercise of a more rapid movement taking place in a symmetrical part of the opposite side of the body. The more rapid movement, on the other hand, is to an extent inhibited.

Will this phenomenon occur if the fingers moved are not symmetrical? To investigate this point *R1* and *L2*, *R2* and *L1*, *R1* and *L4* and *L1* and *R4* were tapped simultaneously.[1]

TABLE IV

Subject *A*								Subject *B*							
R1	155	*R2*	158	*S*	176	3	21	*R1*	173	*R2*	164	*S*	174	9	10
L1	112	*L2*	127	*S*	141	15	29	*L1*	145	*L2*	143	*S*	144	2	1
R1	160	*L1*	125	*S*	148	35	23	*R1*	185	*L1*	150	*S*	164	35	14
R2	161	*L2*	129	*S*	155	32	26	*R2*	178	*L2*	151	*S*	162	27	11
R1	161	*L2*	140	*S*	132	21	— 8	*R1*	181	*L2*	150	*S*	160	31	10
R2	157	*L1*	132	*S*	132	25	0	*R2*	174	*L1*	149	*S*	158	25	9
R1	160	*L4*	89	*S*	93	71	4	*R1*	177	*L4*	133	*S*	144	44	11
R4	107	*L1*	140	*S*	109	33	2	*R4*	145	*L1*	152	*S*	150	7	5
Subject *C*								Subject *D*							
R1	169	*R2*	151	*S*	164	18	13	*R1*	138	*R2*	154	*S*	150	16	12
L1	112	*L2*	115	*S*	121	3	9	*L1*	139	*L2*	131	*S*	140	8	9
R1	185	*L1*	117	*S*	125	68	8	*R1*	138	*L1*	133	*S*	131	5	—2
R2	165	*L2*	118	*S*	130	47	12	*R2*	155	*L2*	133	*S*	133	22	0
R1	193	*L2*	125	*S*	129	64	4	*R1*	135	*L2*	139	*S*	127	4	—8
R2	167	*L1*	126	*S*	120	41	— 6	*R2*	152	*L1*	139	*S*	138	13	—1
R1	195	*L4*	98	*S*	95	97	— 3	*R1*	141	*L4*	84	*S*	111	57	27
R4	113	*L1*	119	*S*	112	6	— 1	*R4*	98	*L1*	139	*S*	106	41	8

In the combination *R1 L2* both *B* and *C* and in *R2 L1*, *B* still show an increase in the rate of the slower finger. *A* has now dropped below the single tapping for *R1 L2* and *D* is below for both. With *R1 L4* and *R4 L1* all the subjects but *C* show an increase in the slower movement. *C*'s simultaneous movement is slightly below that of the slower finger.

In order more readily to examine and analyze these results the difference between the tapping rates of the two fingers

[1] It has not been thought necessary to give a complete table of these tests. The averages were taken from fewer series than the previous ones, but as the general relationship does not vary materially from series to series they can be safely used.

has been placed in the seventh column of Table IV. and the increase in the rate of the slower finger during the *S* movement in the eighth column. A minus sign, of course, indicates a decrease.

Examining first *B*'s result we find that in the asymmetrical pair *R*1 *L*2 there is less of an increase of the slower movement than with the symmetrical pair *R*1 *L*1, an increase of ten as compared to fourteen, but the pair *R*1 *L*4 which is more asymmetrical than *R*1 *L*2 shows a slightly greater increase, *i. e.*, eleven compared to ten. In this latter case, however, the difference between the rates of the two fingers *R*1 and *L*4 is greater than between the former pair *R*1 *L*2. The former is raised one third of the difference, the latter only one fourth of the difference. The result suggests that there are two factors influencing the rapidity of the simultaneous movement, the degree of symmetry and the difference in the rapidity of the two members of the pair. These two factors should act in opposite directions; the difference between the rate of the two fingers increases as a rule with the decrease in symmetry, and the greater this difference in rate the more should the slower finger be aided by the faster in simultaneous tapping, but the greater the assymmetry the less is the advantage of simultaneous tapping. The relation of these two factors very likely differs in individuals. When the coördination is not good asymmetry probably plays an important rôle in slowing the movement. When the coördination is good the rate differences have more effect. Let us examine the data further with this suggestion in mind. Take for example *B*'s *R*1 *L*1 and *R*2 *L*2. The asymmetry is the same but the difference in rate of the *R*2 *L*2 pair is less than that of the *R*1 *L*1 and consequently the increase of the slower movement is less. With *R*1 *L*4 the asymetry is increased but the difference rate is also, so that the actual increase in rate remains the same as the other pair. In the case of *A* with the same two pairs it is true that the results are in the opposite direction, but in the next pair there is a drop in both symmetry and difference and there is in consequence, a falling off in the rate of simultaneous tapping in the one case even below the slower of the pair.

In the most asymmetrical pair, *R*1 *L*4, the difference is very large and there is again an increase of the slower movement notwithstanding the great asymmetry. In *R*4 *L*1, a pair of like asymmetry, there is less difference and less increase. Turning to *D*'s record, we find that although, like the other subject, he showed an increase when the two fingers were of the same hand whether the right or the left, as soon as the movements are on opposite sides there is often evidence of an inhibition. In the *R*1 *L*1 pair we should not expect much change for there is little difference between the rates, but with the *R*2 *L*2 pair, although the difference is twenty-two taps there is no increase of the slower finger, and in the *R*1 *L*2 pair the lack of coördination actually causes an inhibition of the slower movement amounting to eight taps. The results for *R*1 *L*4 and *R*4 *L*1 taken in connection with the foregoing results of this subject, speak strongly for the assumption of the above mentioned opposing factors. The pairs are the most asymmetrical but the differences in rate are very large, being for one almost three times as great as the largest previous difference. Examining the rate for simultaneous tapping we find that the slower movement increases by twenty-seven taps for *R*1 *L*4, that is, the facilitation is 31 per cent. of the rate when the finger is moved alone, and for *R*4 *L*1 there is an increase of only eight but the difference is less, forty-one compared to fifty-seven. The inhibitory effect of asymmetry which, judging from the previous results, is most probably present, has been more than overcome by the facilitating effect of the rate difference. This explanation also fits *C*'s result although he differs in type from the other subjects. It will be found when we come to the further test performed that *C* showed much more pronounced lack of coördination of different muscle groups than the other subjects. There should therefore be less facilitation and probably even inhibition. But he also showed the largest rate difference. Therefore, although the facilitation is less than in the case of *A* and *B*, nevertheless it is present in some instances. In the pair *R*1 *L*4 the large difference of ninety-seven was not sufficient to overcome the inhibition of asymmetry and when

we notice that with the much more symmetrical pair *R*1 *L*2 the large difference of sixty-four was only able to cause a facilitation of four taps, and with *R*2 *L*1 there was a difference of forty-one and yet a decrease of six taps, this result is rather to be expected. The only figures which do not readily offer themselves to the explanation here attempted are those of the pair *R*2 L2. The symmetry of *R*2 *L*2 is the same as *R*1 *L*1 but a difference of forty-seven in the former pair causes a facilitation of twelve taps while a difference of sixty-eight in the latter pair only causes a facilitation of eight. It is true that with none of the subjects is the relation of facilitation to rate difference constant. This ratio also varied with the different subjects. Two points might be mentioned in this regard. First, equality in symmetry does not necessarily mean the same amount of coördination of different muscle impulses. Thus in the results of *C*, although *R*1 *L*1 and *R*2 *L*2 are both symmetrical pairs the coördination between *R*2 *L*2 is better than between *R*1 *L*1, and perhaps for that reason the smaller rate difference has a greater facilitating effect. This explanation could also be offered in regard to the similar results of *A*. Secondly, although the change in symmetry between any two pairs is naturally the same for all subjects, yet one subject will probably have a different change in coördination in going from one pair to another, than a second subject, and this will readily explain individual differences in the above mentioned ratio. It should also be mentioned that investigations on other subjects revealed a difficulty to synchronize, which retarded the simultaneous movement.

Alternating Tapping

As was stated above the alternating tapping was performed in the same series as the simultaneous and the figures for *R*1 and *R*2 may again be used. *A* is the alternation in which one finger ascends while the other descends and *CA* the complete alternation in which one finger does not begin to move until the other has returned. The figures express only the rate of one finger, the number of actual taps made being twice that number. In Table V. these alternations are

expressed in per cent.. of the average of the two fingers tapping singly. Let us first discuss the *A* results. If the fingers were alternating levers of a mechanical machine there would, of course, be twice as much work done in the same time as one lever working alone would perform. In the human machine when two different movements, and in this case opposite movements, are carried on simultaneously we look for some

TABLE V

	Right Hand		Left Hand	
	$\frac{A}{\frac{R1 + R2}{2}}$	$\frac{CA}{\frac{R1 + R2}{2}}$	$\frac{A}{\frac{L1 + L2}{2}}$	$\frac{CA}{\frac{L1 + L2}{2}}$
Subject *A*....	.81	.27	.50	.28
Subject *B*....	.83	.24	.80	.26
Subject *C*....	.37	.16	.54	.23
Subject *D*....	.73	.25	.74	.26

inhibition. The amount of this inhibition is expressed in the per cent. It will be seen that for three of the subjects *A*, *B*, and *D*, with the right hand there is only a loss of about one quarter of the speed of one finger when working alone. In other words by carrying out simultaneously two movements, though opposite in nature, there is a gain of fifty per cent. as compared to the amount of work done if only one movement was performed in the same time. The other subject, *C*, shows a much lower figure. It is only thirty-seven per cent., which means that there is an actual loss in work accomplished by alternating simultaneous movements of thirteen per cent. In his present state of muscular coördination he would accomplish more with one finger moving alone than he would by moving two fingers. Fifty per cent. would mean that the work accomplished by the two fingers is the same as if one had moved alone. The actual figures make the above perhaps clearer. Alternating he only taps fifty-nine times for each finger, or one hundred and eighteen taps in all, while the mean of the rate of the two fingers tapping alone is 160. The loss is forty-two taps or twenty-six per cent. for the two fingers. This subject is the one referred to on page 461 as having poor coördination between different muscle groups.

The above figures make this evident. In this connection it is very interesting to note that *C*'s rate of tapping with one one finger is faster than any of the other subjects with the exception of *B*. From this it would seem that the inhibition is a variable independent of the rate of movement of the separate muscle groups. This assumption is strengthened by the results of the left hand. The separate tapping rates are much lower but the *A* rate is about the same as before, consequently the per cent. is higher. Subject *A* shows this same independence. The *A* rate for the left hand has dropped relatively much lower than the separate rates and the per cent. is consequently lower. In fact it is now about the same as *C*'s rate. That is, his coördination in the left hand is worse than in the right. Subjects *B* and *D* show almost identically the same amount of coördination for both hands. To repeat, the above results substantiate a fact which from what we already know is rather obvious, that the degree of coördination between several muscle groups does not bear any definite relation to the degree of efficiency of the separate muscle groups concerned.

Particularly in regard to the coördination of these movements it seems of interest to inquire into the musical training of the subjects. *A* is proficient with the violin, *B* has played the organ since boyhood, *C* has just begun to take piano lessons, and *D* has played the piano for years for his own amusement. *B*'s ratios of 83 per cent. and 80 per cent. are the highest of any subjects and one is disposed to say that this is due to greater practice and that *C*'s low ratio is due to lack of training. *A*'s figures are what one would expect. Being a violinist the fingers of the left hand are trained to a different set of movements. In playing the fingers are bent and one is held down while the other taps. There would therefore be an inhibition when the fingers were forced to tap alternatingly. This would account for the 50 per cent. which is even lower than that of the untrained *C*. These results, then, seem to indicate degrees of practice but they are too few to be more than a suggestion.[1]

[1] O. Raif argues that the fastest rate required by any piece of music is slower than the average rate of tapping, and therefore piano practice does not increase the rate

There remain to be examined the results of complete alternation (*CA*). Again using the illustration of a mechanical machine the one lever begins to move after the other has stopped. The work done in a given time is the same as if there were only one lever which moved continuously instead of alternating with the second lever. Each finger carrying out such a movement should do fifty per cent. of that which it would do if working continuously. Instead it will be seen that all the subjects with the exception of *C* do only about twenty-five per cent. whether with the right or left hand. *C*'s loss is again greater than that of the other subjects for the right hand. This difference is less than in the *A* rate and with the left hand he shows almost the same per cent. of loss as they do. These figures mean that in this alternation there is a fifty per cent. loss in muscular work done. This amount of loss seems to be relatively the same for both the right and left sides.

Index of Righthandedness

The indices of righthandedness of the first and second finger in the performance of the different combinations here investigated are given in Table VI. As has been done both by Woodworth and by Wells[1] the index is obtained by dividing the rate of tapping of the left hand by that of the right, thus giving the ratio of the efficiency of the two sides. As has been found in the tapping with the whole hand there are great individual differences. The range is even greater than that found by Wells. It is significant that subject *C*, who showed poor coördination in the more complex movements has also the lowest *L*1/*R*1 index which means that he is also relatively inferior to the others in this simpler coördination for the left

but rather the proper timing of the movements. He says: "Nicht in der Bewegung an sich, sondern in der Rechtzeitigkeit der Bewegung, d. h. in dem Zeitverhaltniss von einer Bewegung zur anderen liegt die Schwierigkeit. Diese Rechtzeitikgeit kann zweifellos nur ein Product unseres Willens sein, wir haben also den Ausganspunkt für die Fingerfertigkeit in den Centraltheilen unseres Nervensystems zu suchen, etc." ('Ueber Fingerfertigkeit beim Clavierspiel,' *Zeitschrift für Psychol.*, 24, 1900, p. 354.) While upon the subject of characterization it is worth mentioning that *B* has the fastest tapping rate and *D* the slowest, and both are very athletic, as was mentioned above, which means that here there is zero correlation between strength and rate of tapping.

[1] 'Normal Performance in the Tapping Test,' *Am. Jour. of Psychol.*, 19, 1908, p. 446.

hand. The indices for the efficiency during the first and second half year's work have been given separately under each subject, and we notice that the change is not great. Of these indices five are slightly lower, two are the same, and one higher. That is, there is an indication that practice has had somewhat more effect upon the right hand than the left.[2] The in-

TABLE VI

Subject	$\frac{L1}{R1}$	$\frac{L2}{R2}$	$\frac{(L)A}{(R)A}$	$\frac{(L)CA}{(R)CA}$	Subject	$\frac{L1}{R1}$	$\frac{L2}{R2}$	$\frac{(L)A}{(R)A}$	$\frac{(L)CA}{(R)CA}$
A...	.72 .78	.80 .80	.46	.78	*C*.....	.66 .63	.76 .71	1.03	1.04
B...	.83 .81	.87 .85	.82	.92	*D*....	1.00 .96	.85− .86+	.93	.97

dividual characteristics, however, remain unaltered, the ranking of the subjects according to the size of index being the same. With three of the subjects the $L1/R1$ index is somewhat lower than the $L2/R2$ index due to the superiority of the efficiency of the index finger of the right hand. This is not the case with subject *D*, whose *R2* is throughout decidedly the most efficient finger.[1]

In the *A* and *CA* movements subject *B*, who had the best coördination, shows an index similar to his index for simpler movements. Subject *A* has the same for *CA* but as shown above his coördinated wth the *A* movement on the left side was poor and his *A* index is therefore much lower than his other indices. Subject *C* has the same great difficulty with both hands and consequently has practically no index. Subject *D* has a higher index for the *CA* movements. From

[2] Wells's results are similar to these. He writes: "Again, in neither subject does the left hand show an improvement relative to the right. In Subject *I* the index of right-handedness remains practically the same. In Subject II. the right hand may even improve more than the left." *Loc. cit.*, p. 454. Our subject *A* with one of the fingers of his left hand showed an improvement relative to the right-hand finger. Whipple remarks, *loc. cit.*, p. 143, that "practice affects the left hand no more than the right; consequently the index of right-handedness is unaffected by repetition of the test." This generalization is not borne out by all of the subjects in this experiment, nor by all of Wells's subjects.

[1] The subject could give no reason for this. He had never to his recollection exercised this finger more than the others, and believes it must be an inate characteristic.

these results we again see the low correlation of these complex coördinations with the simpler coördinations of the single movement as was shown before.

Practice Effect

The experiments were not arranged with the idea of investigating practice and fatigue, but it does not seem amiss to discover what evidence there is of their effect under the conditions described. An examination of the tables I., II. and III. shows as was to be expected from the results of previous work that the improvement is not a steady one. If curves were plotted they would reveal the characteristic fluctuations. As stated, the maximum rate for each series is in heavy type. See Tables I., II. and III. It will be seen that it may occur at almost any point of the series, nor is the R_1 maximum necessarily obtained on the day of the R_2 maximum or the S maximum on the day of the maximum of either finger concerned, nor does the A maximum always occur on the day of the AC maximum. In short there is a low correlation as regards the days of the maximum results for the different fingers of the different movements.

Table VII. has been arranged to show the general change between the first and second half-year's work, and between the first and second part of the first half year. The figures in the first and second horizontal columns are the averages for the first and second half of the series given in Tables I. and II. The third, fourth, and fifth horizontal columns contain the result of the second half year's work.

It will be seen that with the separate tapping of the fingers of the right hand in the case of two of the subjects, B and C, there is decided evidence of the effect of practice, not so much with B in the difference between the first and second half year as in the difference of the halves of the first half year. The other two subjects do not show this difference, in fact A in the first half year shows a falling off. With the left hand fingers B and C again show the effect of practice, but it is not so marked as with the right hand. Subject A shows a practice improvement in the index finger of the left hand,

and subject *D*'s results again show no evidence of practice effect. In the alternating movements *B* shows decided improvement as does also *A*, while *C*, who had great difficulty, shows a considerable loss. *D* remains about the same. In the complete alternation there is no significant changes. The change in the simultaneous movement follows the change in the individual movement.

The most evident fact in these results is the wide individual differences which preclude any general statement. It is

TABLE VII

Subject	*R*1	*R*2	*A*	*CA*	*S*	*L*1	*L*2
A.....	158	168	121	41	184	112	127
	152	149	133	43	168	125	129
	160	161					
	160						
	161	157					
B.....	169	154	136	41	168	145	143
	180	176	148	41	181	150	151
	185	178					
	177						
	181	174					
C.....	157	143	68	27	158	112	115
	187	163	59	25	174	117	118
	185	165					
	193						
	195	167					
D.....	140	159	106	34	152	139	131
	136	155	107	37	148	133	133
	138	155					
	141						
	135	152					

interesting to note, however, that practice does affect the result in some cases even though the daily amount of work of each finger is slight and there is a week's interval, if not more, between each series.[1] Another fact to be noticed is that the practice gain in the more complex coördination, as in the case of the *A* movement, may be independent of the progress of a less complicated movement. In the case of subject *A* both *R*1 and *R*2 show a loss in rate in the second half of the series, *R*2's loss being considerable, and yet the *A* movement,

[1] The difference of practice gain between the two hands appeared in the difference of indices for right-handedness, p. 466.

which is a coördination of these two, shows a decided gain. Subject *C* shows a gain by practice in *R*1 and *R*2, and a loss in the *A* movement. Finally the fact that the more voluntary movement, *CA*, shows practically no change in rate, should be emphasized.

The two horizontal columns next to the last column in Tables I. and III. are arranged to compare the averages of the two orders of procedure. The first one of these horizontal columns for each subject gives the averages when a single finger movement precedes the double, simultaneous movement, the second when the reverse order is used. In both series the rate for the *R* finger for all subjects except *C* and one figure for *B* is more rapid when the *R* succeeds than when it precedes the *S* movement. In the majority of cases the difference is as large or larger than the m.v. Only in two instances is the rate for the *L* appreciably affected by the order and the results are of opposite nature. The simultaneous movement in three cases is decidedly more rapid when it starts the series, in two instances the difference is greater than the m. v. The only results, then, that seem at all significant are those that show the *R* movements more rapid when they succeed the other movements. The possible explanation is that with this finger the warming up effect was greater than the fatigue. It must be remembered that there was a pause of two minutes between the 30-second tests which could very well be sufficient for recovery of this finger but not for the others. In the wrist-tapping test Wells used two and a half minute pauses and his results show an increase in rate as the series progressed.

Fatigue

The amount of fatigue in each 30-second series is calculated by finding the relation of the difference in rate of the first and second 15 seconds to the entire 30 seconds. These were found for the results in Tables I. and III. and are given in decimal form in table VIII. The absolute differences are also given. For instance, the first figure in column 3, *i. e.*, .047, was found by dividing the rate of *R*1 which is 155 into 7.3 which is the

difference of the two 15-second halves.[1] The averages for all the subjects are given in column 10.

Most striking is the fact that fatigue is greatest for the *A* movement and that there is no fatigue in the *CA* movement. It must be remembered that the *A* movement is the rapid antagonistic movement, the *CA* movement is very slow and one set of muscles rests while the other reacts. This readily explains

TABLE VIII

	Subject *A*		Subject *B*		Subject *C*		Subject *D*		Av.
*R*1.....	7.3	.047	6	.034	12	.071	6	.044	.049
*R*2.....	6.5	.041	4.5	.028	8	.053	9	.059	.045
A.....	9	.071	7	.05	4	.07	6	.056	.062
CA.....	—1/8		—1/18		—2/5		—1/3		
S.....	7.5	.042	5	.028	5.5	.033	6	.04	.036
*R*1.....	4	.025	5	.027	11	.059	6	.043	.039
*L*1.....	5.5	.044	4	.027	5	.043	7	.053	.042
S.....	2.5	.017	5.5	.034	4.5	.036	6	.046	.033
*R*2.....	5	.031	5	.028	7	.042	10	.065	.041
*L*2.....	5	.039	5	.033	2	.017	6	.045	.033
S.....	6	.039	5	.031	6	.046	3.5	.026	.035

the absence of fatigue in 30 seconds. This is not only true for the averages but with few exceptions for all the subjects. For subject *C* the *A* movement fatigue is about the same as the *R*1 fatigue. The *S* movement for *R*1 and *R*2 for two subjects is the least fatigueable, for the other two it is the same as the lowest index of the single finger. In the *S* movement for *R*1 and *L*1 and *R*2 and *L*2 the index is the lowest in three instances and only twice is it higher than both of the single movement indices.

It may be said, therefore, that in general the simultaneous movement of two fingers for 30 seconds does not show more, and very often less fatigue than one finger. A comparison of *R*1 and *R*2 in the two halves of the table shows as other experiments have before, that practice has the tendency to reduce the fatigue. Only in one instance out of eight is it

[1] In view of the manner in which the results were recorded, it was thought better to divide the periods in halves rather than to compare the first five seconds with the averages of the 1st, 2d, 3d, 4th, 5th and 6th five second periods as Wells did. *Loc. cit.*, p. 469. His index is higher probably because the initial spurt has thus more influence on the result, it being reduced in the above index by the results of the 2d and 3d five-second periods.

greater with practice. As has frequently been found the decrease of fatigue is the important factor in practice gain. No correlation can be found between the fatigability of the two hands. Nor can a general statement be made as to the fatigue index of the right as compared to the left hand. In three subjects the relation between *R*2 and *L*2 and *R*1 and *L*1 in regard to fatigue is in the same direction, except that subject *B* shows the same fatigue for *R*1 and *L*1 while *R*2 is less than *L*2, but subject *D* shows opposite results. For him the fatigue for *L*1 is greater than for *R*1 and for *L*2 less than *R*2. One cannot say, therefore, that if the *R*1 finger is more easily fatigued than the *L*1 finger, that the *R*2 finger will be more easily fatigued than the *L*2 finger.

Variations.

There is less variation in the left-hand movements than in those of the right hand. This is what both Bryan[1] and Wells[2] found for wrist movements. The small m. v.'s accompany the slower reactions but there are even indications of a less relative variation on the left side. The simultaneous movement shows about the same variations as the single movements. The *CA* movement shows the least variation of all the movements. This movement being very slow (fewer taps per 30″) the relative variability is higher than with the other movements. No general statement can be made in regard to the *A*-movement. There is a tendency for it to be relatively larger than that of the single movement. Absolutely it is sometimes larger and sometimes smaller.

Summary and Conclusion

If the index and second finger of the right hand are tapped simultaneously as rapidly as possible the resulting rate according to the results of four subjects of varying degrees of motor ability, is faster than the rate of the slower finger and may even be more rapid than the faster finger when tapping singly. There is doubtless a more or less innate coördination

[1] *Loc. cit.*, p. 163.
[2] *Loc. cit.*, p. 480.

between the movements of the fingers of the same hand caused by the biologically important grasping reflex. When the extensor of the index finger is innervated a tendency for the symmetrical extensor of the next finger to move is also observed.[1] Ths impulse must be inhibited and it is probable that this inhibition also extended to the motor half-center of the first finger causing a loss in rate of movement. An inhibition somewhat similar in nature has been demonstrated by Sherrington in his experiments on the stepping reflex when he simultaneously stimulated two afferents which are antagonistic in their effect. He says: "Of the two afferents concurrently stimulated, that one which when stimulated alone causes flexion of the joint excites the flexor half-center and inhibits the extensor half-center; and the other afferent, which when stimulated alone causes extension of the joint, excites the extensor half-center and inhibits that of the flexor. When both afferents are stimulated simultaneously with appropriate intensity, the discharge from the flexor half-center represents the algebraic sum of the opposed excitation and inhibition which the two afferents individually exert on it, etc."[2] In our experiment it is the inhibition of one flexor half-center which is communicated to the other flexor half-center and this inhibition is then compounded algebraically to the excitation of the extensor half-center. When both fingers move simultaneously this inhibition is removed. This at least seems to be a plausible explanation of the fact that the simultaneous movements were faster than either single movement. Whether it was the only factor could readily be tested by tapping simultaneously symmetrical fingers on two hands. It was found that under these conditions the movement of the slower finger was facilitated, the degree varying with different individuals. The two fingers together, however, never tapped faster than the faster finger.

[1] L. Huismans states "Homolaterale M–B. (Mit-Bewegungen) sind in den weitaus meisten Fällen auf eine Irradiation des Bewegungsimpulses in der Hirnrinde Zurückzuführen" ('Über Mitbewegungen,' *Deutsche Zeitschrift für Nervenheilkunde*, 40, 1910, p. 233).

[2] 'Reflex Inhibition as a Factor in the Coördination of Movement and Postures,' *Quarterly Journal of Experimental Physiology*, 6, 1913, page 269.

Numerous examples are to be found in the literature relative to the influence of the movement of one side of the body upon that of the other. In the simple reaction experiment Paul Salow found that the reaction time for simultaneous movements of two hands was longer for each hand respectively than when they reacted separately.[1] M. L. Patrizi found in his ergograph tests that in simultaneous action both hands did less, but when they worked alternately the right hand action reinforced that of the left hand. He believes that one cannot give maximal attention to the two simultaneous acts.[2] W. W. Davis found that in general the right hand tapped more rapidly alone than in connection with either the other members. Two of his subjects were able to tap more rapidly when all four members were tapping. He remarks that "with a longer practice the right hand, in multiple tapping, would undoubtedy excel in rapidity its record while tapping alone." He also concludes from his results that "there is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or position." In this work Davis was not interested in the influence of the faster on the slower member.[3] Mention should also be made of the fact that a paralyzed member may be moved by moving a healthy member.[4]

[1] 'Untersuchungen zur uni- und bilateralen Reaktion. II. Einige Versuche am Chronographen,' Psychol. Stud., 8, 1913, pp. 506–540).

[2] Patrizi writes: "Les recherches que j'ai rapportées dans ce mémoire nous font admettre une uncompatibilité d'états psychiques, même quand il s'agit de la coïncidence, dans le même instant, d'impulsions maximales destinées à des mouvements symétriques et homogènes et qui sont habituellement accouplés." In regard to Féré's results, which indicate that when the left hand is almost fatigued its capacity can be increased by the movement of the right hand, he says that an increase will not occur if the two movements are exactly simultaneous ('La simultanéité et la succession des impulsions volontaires symétriques,' *Arch. Italiennes des Biol.*, 19, 1893, p. 138).

[3] 'Researches in Cross-Education,' Yale Studies, 6, 1900, pp. 6–50.

[4] See J. Grasset, 'L'action motrice bilatérale de chaque hémisphere cérébral,' *L'annee Psychol.*, II., 1904, pp. 434–445. It is interesting to note that W. P. Lombard said some years ago: "Not enough work has been done to admit definite statements concerning the strengthening or weakening effect of the action of the one hand upon the other. . . . The few observations which have been made with reference to this question favor the idea that if one hand acts simultaneously with the other it tends to weaken rather than strengthen its movements. This effect is not a constant one, however, as

In searching for an explanation for this contralateral facilitation of the slower movements by the faster, the fact of sympathetic movement seems the most significant. Everyone has had the experience when the member which one desires to move is held or when it has become fatigued, of moving the opposite member. In this regard Ch. Féré remarks that the examples drawn from his research "indiquent que, lorsqu'il existe un obstacle à un mouvement volontaire unilatéral, l'influx nerveux a une grande tendance à prendre la voie symétrique du côté opposé." This tendency is greater in children, being later more or less suppressed.[1] In the simultaneous movement of the two fingers the faster finger must be held back to synchronize with the slower. According to the above if the one finger were not already moving while the other finger was being held back from its full movement the sympathetic movement of the former would probably be evident. When it is moving at the same time as the finger which is being somewhat retarded, its movement is facilitated by the surplus energy of the faster finger. There is also another explanation or, perhaps, a second factor in conjunction with the above and that is the increased amount of peripheral stimulation, *i. e.*, the contact with the board and bar. We know from the work of Sherrington, Alexander Forbes, T. Graham Brown, and others that the afferent impulse on one side of the body may cause a contralateral reflex. Now the afferent path on the ipsolateral side may be fatigued and the stimulus on the contralateral side may become more effective. Alexander Forbes says: "The fact that central fatigue induced through one afferent nerve usually does not impair the reflex involving the same muscles induced through another afferent nerve supports the conclusions of Lee and Everingham that this fatigue does not involve the moto-neurones, and accords with the view of Sherrington that its seat is the synapse."[2] This explanation

many exceptions occur" ('Alterations in the Strength which occur During Fatiguing Voluntary Muscular Work,' *Jour. of Physiol.*, 14, 1893, p. 116).

[1] L'alternance de l'activité des deux hémisphères cérébraux,' *L'Année Psychol.*, 8, 1901, p. 148.

[2] 'The Place of Incidence of Reflex Fatigue,' *American Journal of Physiology*, 31, 1912–13, p. 122.

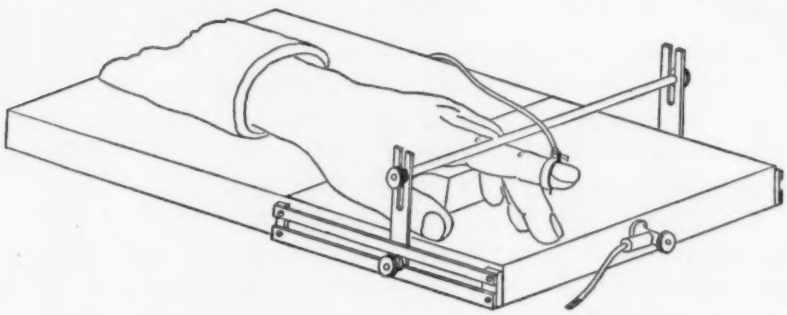
can also be applied to simultaneous tapping on the same hand. Owing to the fact, however, that these movements were never carried on sufficiently long for any great fatigue this factor must play, if at all, a very small rôle.

Thus far we have been discussing simultaneous movements of symmetrical members. This facilitation was also observed between asymmetrical members but it was not so great. There can be little doubt that the coördination is less perfect the more asymmetrical the members are. Even the transfer of practice is greater to symmetrical parts.[1] There is not, however, a very high degree of correlation between increasing symmetry and increasing facilitation with all the subjects. In order to explain the results an antagonistic factor has been suggested, *i. e.*, that, ceteris paribus, the greater the difference within limits between the rates of the two fingers the greater the facilitation of the slower. This difference increases with the asymmetry and at times causes an asymmetrical pair to show greater facilitation than a more symmetrical pair. An explanation which includes two opposing factors can explain anything and should not be used without strong proof. There are, in this series of experiments, instances where the degree of symmetry is a constant and we see here the effect of difference in rate in the direction just mentioned. We have also cases of similar degrees of difference in rate and here the effect of symmetry can be seen. The results are not sufficiently consistent to be conclusive. They are, however, suggestive and the explanation founded upon them must necessarily also bear that adjective.

2. An examination was also made of the simultaneous movement of the index and first finger of the right and left hand when these movements were in opposite directions, a combination of movements similar to the stepping reflex and spoken of in this paper as alternating movements. Here there is double reciprocal innovation according to Sherrington. The four subjects were divided into two groups in respect to the amount of coördination. With three of the subjects each finger was able to do about three quarters as much in this

[1] See W. W. Davis, *loc. cit.*, page 49.

combination as when tapping alone, except for one of the subjects with the left hand. The loss for each finger due to this combination of impulses was only 25 per cent., so that in the same time the pair was able to do one and a half times as much as a single finger. The fourth subject showed a much poorer state of coördination. Two of the three subjects with good coördination were piano players and the other who had a low efficiency with the left hand had his fingers of that hand trained for another system of coördination, for violin practice. Judging, however, from the great differences be-



tween these subjects and from observations on others, it seems probable that this test shows some fundamental differences in motor coördination. It would appear profitable to gather further data with special reference to the effect of practice, the tests here described being too few to draw any conclusion from them on this point.[1]

Another fact which appears clearly in the results is that the amount of coördination between the two reflexes does not run parallel with the development of these reflexes. The subject who has the worst coördination has one of the highest

[1] Sherrington, in speaking of grace in walking, says that "the proper execution of the act ensures a moment of complete rest to each of the opposed motor centers engaged." *Loc. cit.*, p. 271. T. Graham Brown found very decided differences in the reflexes of the cat " . . . these individual variations are probably due in great part to more fundamental differences in the constitution of the nervous centers. Some cats are 'walkers.' They exhibit in a marked degree the phenomena of 'narcosis progression.' Other cats are 'scratchers.' In them the scratch-reflex is peculiarly excitable" ('Studies in the Physiology of the Nervous System, XIV. Immediate and Successive Effects of Compound Stimulation in Spinal Preparations,' *Quarterly Journal of Experimental Physiology*, 7, 1914, p. 200).

rates in single tapping. He is a subject with little piano practice, so that this ability in single tapping substantiates Raif's assumption (vide supra, p. 464) that piano players do not show any special ability in rate of tapping.

3. The term complete alternation has been applied here to successive movements of the two fingers; one finger completes both the extensor and flexor movements before the other begins. With all the subjects and with both right and left hand the two fingers in this combination tap only one quarter the amount that they do in the same time in tapping continuously and alone. The loss in this form of alternating movement is about 50 per cent. This combination is a system of successive reflexes. The loss in efficiency is probably in great part due to the necessary inhibition of the previous reflex. As Sherrington says ". . . there will persist during the new reflex activities belonging to the old with, in result, confusion of the two. Rarely, indeed, can it happen normally that the reflex machinery in executing a train of different reflexes is actuated by a train of different stimuli, each one of which abruptly ceases just as the next one begins."[1] And further: "For orderly and unconfused sequence of reflex acts—also of willed acts—central inhibition is a necessary element of coördination in the transition from one muscular act to another."[2] Another reason for the loss is that in the single tapping movement inhibition of one of the antagonistic movements increases the tendency for that reflex to discharge, causing what might be termed a rebound. If, however, the finger must rest after the completion of each flexor movement until the other finger has completed its movement, most of this post-inhibitory effect is lost.[3]

4. Even with an interval of one week between the tests a practice gain in most of the movements is noticeable. An exception must be made with the most voluntary movement, namely, the complete alternation. The subject who had the worst coördination in the alternating movement even showed a loss. The practice effect in the alternating movement does

[1] *Loc. cit.*, p. 275.
[2] *Loc. cit.*, p. 276.
[3] See Sherrington, *loc. cit.*, p. 278.

not run parallel to the practice effect of the fingers tapping separately. A gain in the former may be accompanied by a loss in the latter. Practice in the simultaneous movement, on the other hand, does follow that of each finger when working separately. There is some evidence that practice affects the right hand more than the left.

5. Fatigue is noticeable for all the movements except the complete alternation.[1] It is greatest for the alternating movement and generally least for the simultaneous movement. That is, during thirty seconds there is less fatigue when two fingers are working simultaneously than when one is working alone.

6. The index of righthandedness is not necessarily the same for all the movements, nor is it always the same between the different pairs of symmetrical fingers.

7. The variations in tapping rate are less for the left hand.

[1] Wells says: "To sum up, the maximum rate of repeated voluntary movements is a function that practically every investigator working with sufficiently accurate methods has found to be subject to fatigue effects, though the degree of this subjection has differed considerably" ('A Neglected Measure of Fatigue,' *Am. Jour. of Psychol.*, 19, 1908, pp. 352–3.)

RETINAL FACTORS IN VISUAL AFTER-MOVEMENT

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The present paper is a continuation of a previous study made by myself on the after-effects of visual motion.[1] On pages 255–257 of that article, comments are to be found bearing upon retinal factors effective in the production of the illusory motion which occurs after gazing for some time at a series of moving bands. Again on pages 275 and 276 an experiment is described in which a moving area was used of a size sufficient to cover most or all of the visual field. In this test an after-movement was observed which went in the same direction as the real or stimulus movement. Ordinarily the after-effects seen are secured with small stimulus areas and move in a direction contrary to the real movement. In the earlier study, it was held that the normal after-effect was a result of some or all of the following factors: eye-muscle strain, association factors and retinal processes which were probably fading after-images. The present work is primarily concerned with a determination of the nature of the effective retinal factors. The conclusion reached is that this factor is a *streaming phenomenon* which moves through external space in a direction opposite to that taken by the stimulus area. The evidence for this is necessarily of an introspective and theoretical nature.

The data presented were obtained largely from three subjects, two of whom had served in the earlier tests. A number of observations were made on untrained subjects. The apparatus used was the large striped curtain (six feet long by four feet high) described in the earlier paper. This curtain could be made to move either up or down. A small square of white paper was held close in front of the curtain by a thread and served as a fixation point. A number of

[1] Hunter, Walter S. 'The After-effect of Visual Motion,' Psych. Rev., Vol. 21, pp. 245–277, 1914.

newspapers were tacked on the wall above the curtain to serve as a projection field for the after-effects. Other fields will be mentioned later. Tests were also made with rotating black spirals and with a striped Scripture drum.

Experimental Data

The immediate problem from which the present tests took rise was that of the occurrence of an after-movement (abbreviated as a.–m.) in the same direction (an s.–a.–m.) as the stimulus movement. It will probably conduce to clarity, if the observed facts are grouped accordingly as this type of after-movement is or is not seen. The facts concerning streaming can then be presented.

Observations on S.–a.–m.—If an observer is seated about one meter from a moving curtain of the type used here and fixates a point in front of the curtain for twenty seconds, upon turning his eyes to a series of newspapers tacked upon the wall above the curtain, he will usually see the following phenomenon: the fixation point of the projection field and probably about one or two square feet of the immediately adjacent paper will be seen to move in the same direction as the real or stimulus movement. This movement is most rapid during the first second and is always a drifting bodily movement of the projection field. It is identical in quality or kind with the usual after-movement seen when a small stimulus area (parallel stripes on the Scripture drum or the Archimedian spiral) is involved and when the projection field is the stopped stimulus area. The difference between the two phenomena is one of direction only.

I have never secured an s.-a.-m. using the stopped curtain as a projection field. (The same is true for spirals and for striped fields of small extent.) It is all but impossible to secure it if the observer sits within eight inches of the moving curtain even though he projects the after-effect above the curtain as before. When the observer sits some two and a half meters from the moving curtain, he can secure the s.-a.-m.; but in the tests reported here, it has not been so easy as from the distance of one meter.

The following test has also been made. A white cardboard 11 in. × 14 in. with an aperture 4 in. × 7½ in. was placed in front of the moving curtain. An observer seated at from one to three meters fixated a point on the edge of the aperture for twenty seconds and then fixated a dot on the papers above the curtain. A stationary black negative after-image of the cardboard was seen. In this a.-i., there was a rushing movement either upward or downward in the same direction as the real or stimulus movement. Outside the black a.-i. and in the area corresponding to the aperture of the card, a movement is usually to be seen which goes in a direction opposite to the real movement. (This we shall term an op.-a.-m.) Some observers reported the whole projection area to be involved in an s.-a.-m. When the after-effect was projected upon either a black or a white cardboard, no movement was seen in the negative after-image of the cardboard which had been in front of the curtain.

Observations on Op.-a.-m.—An after-movement in the opposite direction to that of the real movement is the usual and "normal" after-effect save under the circumstances just described. When the moving area of large extent is used and the after-effect is projected upon newspapers on the wall above the curtain, an op.-a.-m. is seen particularly below and to the sides of the fixation point. Simultaneously the s.-a.-m. described above is seen around the fixation point. At times the op.-a.-m. appears later than the s.-a.-m. Some observers see a drifting op.-a.-m. around and over the fixation point also. In this case the movement seems to be between the observer and the paper. These central and peripheral op.-a.-ms. are nearly always described as a film moving over the projection field and not as a movement of the field itself.[1] It is a radically different type of movement from the s.-a.-m. seen about the fixation point. The op.-a.-m. is a film which is either described as a series of shadows or as a 'rainfall' or 'sleet' or 'dust film.' Frequently this film will drag the projection field along with it; but even in this case there is no drifting bodily movement as is seen in the s.-a.-m.

[1] See Hunter, *op. cit.*, pp. 247–8 for results of earlier tests.

If the projection field is the stopped curtain, an op.-a.-m. is seen which is a slow drifting movement of the field itself. A film has never been seen by my observers under these conditions in the present tests. This is also true when stimulus areas of small extent are used, whether they are spirals or systems of parallel lines. In the earlier tests just referred to in the footnote, such a film was suggested by the observers. The absence of this in the present tests is probably due to the fact that the subjects were mainly familiar with intense films.

Observation on Streaming.—It will be well to preface these observations with a few historical comments. Our interest lies chiefly in the observations of Pierce,[1] Szily[2] and Ferree[3] who wrote in that chronological order. The present writer does not care to examine any claims as to originality. It is quite probable to his mind that the phenomenon here under consideration has long been known. Schilder,[4] *e. g.*, quotes from Purkinje's 'Beobachtungen und Versuche' passages on the streaming phenomenon that parallel Szily's. Ferree accepts Pierce's explanation as a basis for distinction between their respective observations.

Pierce's observations were made with a stationary black and white striped area. The projection field was a plain black ground of cloth or card. After some twenty seconds' fixation, upon turning to the projection field, "The appearance is that of *a thin cloud of fine white dust moving across the field of vision.*" The direction of moving is always perpendicular to the striped lines. The same observations were verified with concentric circles. Again, "if the usual field of fixation be divided by a vertical strip of some uniform color, no 'drift' will be seen in that portion of the field corresponding to the strip." In explanation Pierce says "it seems probable on the whole that the ultimate explanation of this as of all after-

[1] Pierce, A. H., 'Studies in Space Perception,' pp. 331–8, N. Y., 1901.

[2] Szily, A. v., 'Bewegungsnachbild und Bewegungskontrast,' *Ztsch. f. Psych. u. Physiol. d. Sinn.*, 1905, Bd. 38, S. 124.

[3] Ferree, C. E., 'The Streaming Phenomenon,' *Amer. Jr. Psych.*, 1908, Vol. 19.

[4] Schilder, Paul, 'Über auto-kinetische Empfindungen,' *Arch. f. ges. Psych.*, 1912, Bd. 25, S. 71–3.

images of motion, will be somehow formulated in terms of impulses to movement aroused by the particular stimulation that precedes. Perhaps the experiments here recorded may contribute their mite towards this final explanation, if that ever comes."

Szily's essential statement is made in connection with his discussion of the s.-a.-m. and is as follows: "Die hier geschilderten Nebenerscheinung begleitet, die dem minder unsichtigen Beobachter allerdings erst dann auffällt, wenn sie unter gewissen Versuchsbedingungen in erhöhtem Masse zur Geltung gelant. Ich selbst sehe unter allen umständen in der Peripherie das Regelrechte [op.-a.-m.] Bewegungsnachbild, zumeist in der Hülle eines strahligen Nebels, in entgegengesetzter Richtung ablaufen. Bedient man sich eines noch dichteren Streifenmusters, verlängert man die Dauer des objektiven Eindruckes, setzt man die Beleuchtung des Projektionsgrundes herab, so verbreitet sich diese Zone des regelrechten Bewegungsnachbildes mit gleichzeitig erhöhter Intensität immer mehr zentrumwärts, so dass sie selbst dem Ungeübten auffallen muss. An den Konturen im Bereich des direkten Sehens aber, solange sie als wahrgenommen werden, vollzeiht sich auch dann noch stets die paradoxe [s.-a.-m.] Scheinbewegung."

From Ferree we may take the following: "When one sits with lightly closed lids, which must be kept from quivering, before a bright diffuse light such as that of a partly clouded sky, and looks deep into the field of vision thus presented, beyond the background as usually observed, one sees about the point of regard, after the field of vision has steadied, slowly moving swirls. These swirls have the appearance of *streams of granules*[1] moving in broad curves now this way, now that, seemingly without order, unless a noticeable eye-movement occurs, or is made voluntarily, when the direction of streaming changes to that of the eye-movement." Ferree is inclined to identify the phenomenon with a diffusion of lymph over the retina. Aside from the above general description, our interest centers in the drawings which his subjects made. These

[1] Italics mine.

represent the qualitative character of the phenomenon and are identical in everything save direction with those made by my subjects and represented in Fig. 1.

The quality of the streaming observed by the subjects of the present tests was either of two kinds: (1) 'a shadow-like succession of clouds' of rather medium velocity; or (2) a 'sleeting,' 'snow-fall' or 'dust streaks' which went usually at a high velocity. In any case, *this streaming is always in a direction opposite to that of the real or stimulus movement.* Neither type appeared in these tests when the projection field was the stopped stimulus area. The first type has never appeared when the projection field is a plain black or white cardboard. It has been observed in the periphery where the after-effects have been projected upon the paper above the large curtain, and all over the visual field when the after-effect has been projected upon floors and upon plain gray walls.

The greater theoretical significance attaches to the 'sleet' film. This will appear as the discussion advances. I have assumed that the best method of detecting what takes place in or on the retina is to project the phenomena upon black or white surfaces (cardboard, shadows or the black of the retinal field). Under these conditions the phenomena appear unmodified by variations of the external world.

A 'dust' or 'sleet' film—which is best represented by *A* in Fig. 1—is always seen if the after-effect of the moving curtain is projected upon plain surfaces as just described. In these cases it is pure, *i. e.*, unmixed with the first type. Pierce's test was carried out with the present subjects. The stimulus areas were the large striped cloth, the striped Scripture drum and a black spiral on a white disc. In each case after a fixation of twenty seconds, the observer turned his eyes to a black or white ground and described the phenomena seen. Drawings of the after-effects were requested. When the stimulus area was one of parallel stripes, the after-effect was a 'dust film' as described by Pierce. It differed from that secured with a moving stimulus area *only in its fainter intensity and in its less certain direction.* By the last

phrase, I mean this: the Pierce dust film was perpendicular to the lines, but the film could be interpreted as moving either up or down. With the dust film produced by a moving stimulus area, there was no doubt but that the film moved in a direction opposite to that of the stimulus area. The drawing *B* in Fig. 1 represents the phenomenon seen on a plain card after having fixated either a stationary or a rotating spiral for twenty seconds. The difference just described for parallel

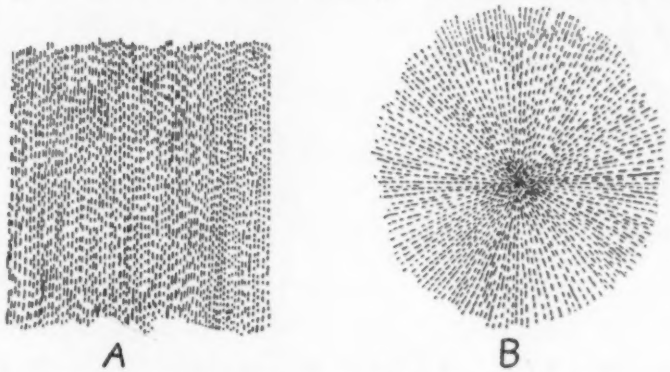

FIG. 1. Patterns of Streaming Observed in the Present Tests. *A*. Seen with either a moving or stationary area of parallel stripes when the projection field is a plain surface. *B*. Seen with either a moving or a stationary spiral when the projection field is a plain surface.

lines is the only one that holds here also. In each case the film has appeared before the negative after-image of the stimulus area.[1]

Earlier in this paper, tests were described where a white cardboard with an aperture was placed in front of the large moving curtain. In order to analyze the nature of the retinal factors producing the peculiar after-effects already described, the phenomenon was projected upon a black card. In the black negative after-image of the white card (where the s.-a.-m. had been under other conditions), there was no movement; but in the remainder of the card which corresponded to a stimulated retinal area, the dust film was seen (moving of course in a direction opposed to that of the stimulus movement). There can be little doubt, then, but that the

[1] See Hunter, *op. cit.*, p. 258.

s.-a.-m. seen in the black after-image when this was projected upon the newspapers was due to association factors. The movement of the film produced the impression of a movement of the objects within the after-image in an opposite direction. Szily[1] describes a similar experiment under the caption 'Kontrast im Bewegungsnachbilde.' I am uncertain how he would explain it. However other cases of 'contrast' are accounted for on the basis of a higher threshold for the perception of movement in central than in peripheral vision. This, I think, in all of the cases described, is a needless hypothesis.

The author has repeated some of Ferree's tests upon himself and one of his subjects. It has been possible to confirm the existence of a normal streaming activity in the eye. This when represented by drawing is exactly similar to the pictures of dust films found in the present experiments upon stationary and moving striped areas.

Theoretical Considerations

To keep theory close to observed fact, it seems to the present writer that the choice of explanatory retinal processes lies between a "streaming phenomenon" and fading after-images. The latter is the more conventional and therefore the more apt to be favored. The evidence in its favor, however, is purely hypothetical and non-observational. Hence as an hypothesis it may well be faulty. What is actually seen when the eyes are either closed or turned toward a uniform field is a dust like film in constant movement. To pass from Ferree's streaming to Pierce's streaming, it is necessary to assume: (1) that the after-image effects of the striped field make the dust film more vivid; and (2) that in some manner the striped field gives definite direction to the streaming. I can offer no solution for the last statement, although it is by no means absurd and impossible. Concerning the first assumption, there can be no difficulty for experiment shows that the film can be more readily seen upon certain backgrounds than upon others. This will also account for the fact that the after-effect is largely confined to (*i. e.*,

[1] Szily, *op. cit.*, S. 126.

visible on) an area corresponding to that stimulated by the real movement.

Let us examine each proposed retinal factor with the aid of Fig. 2. Of the phenomena described above, it is necessary to bear in mind particularly the s.-a.-m. about the fixation point and the dust-like film which is seen to pass over the projection field in a direction opposite to that taken by the stimulus area. Our discussion will be further aided by a quotation from Wundt giving the conventional statement of the after-image theory. "Indem ein schwaches Nachbild der gesehenen Bewegung im Auge zuruckbleibt, scheint ein fixiertes Objekt infolge der Relativität der Bewegungsvorstellung in entgegengesetzten Sinne bewegt zu sein. Das Nachbild, in der Regel zu Schwach um selbst gesehen zu werden, genügt doch um auf das Objekt die zu seiner eigenen entgegengesetzte Richtung zu überträgen."[1]

In Fig. 2, *O* represents the curtain moving up. i. is its

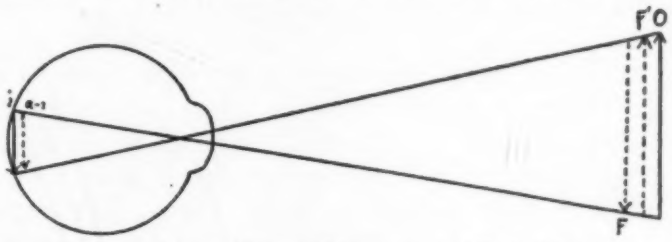

FIG. 2. Relations between External Movements and Retinal Processes.

image on the retina where the movement passes down. a.-i. stands for the fading after-images which move in the same direction as i. and not up with the curtain, *O*. Let us assume that twenty seconds have passed and the striped curtain or drum is stopped. Upon whatever objects the eye is turned the stationary images of these objects will be cast upon the area i. over which the fading after-images (a.-i.) are passing downward. We have two possibilities now: (1) The fading after-images do not enter consciousness. In this case a.-i. passing down over i. gives the impression of i. moving up, *i. e.*, is the same as though a.-i. were stationary

[1] 'Physiol. Psych.,' Bd. 2, S. 622, 6. Aufl., Leipzig, 1910.

and i. were moving up. i. projected, then, into the external world would be seen as *O* in movement downward. (Here I assume that a sub-liminal retinal process is not projected into space. If it were it would be discussed as though the a.-i. came into consciousness and hence would be treated under (2) below.) The present case can explain an s.-a.-m. about the fixation point by claiming that for some reason the movement of a.-i. on the retina is, relatively, non-effective in central vision. Szily does this by appealing to the high central threshold for the perception of movement. The truth in this point lies only in the observable fact that the direction of apparent movement differs in the periphery and in the center. The explanation of this lies in the fact that the clear bold contours in central vision dominate over the moving process as compared with the peripheral contours. When the after-effect is projected upon plain cardboard the dust film is seen all over the field. The effectiveness of contours in inhibiting the perception of after-movements is shown by Szily,[1] and is continually verified in tests on after-movements. The validity of that investigator's threshold hypothesis is further impugned in the following case. (2) *If the fading after-images do enter consciousness*, they do so as projected into external space. a.-i. is then seen as F′ moving up. *O* seen through this appears to move down. The damning fact, however, is that the film or 'sleet' is actually seen not as *F′* but as *F* and moves down. *It cannot therefore be the projection into space of fading after-images.* The retinal equivalent of the film which is seen to move down over the external objects must be the passage of a stimulation upward. However before ruling fading after-images out entirely, it is well to consider the following possibility: May it not be that a.-i. passing down over i. is the equivalent of a wavering or filmy passing of i. upwards on the retina? This when projected would give *F*. Such an hypothesis appears fairly plausible when *F* is projected upon print or other equally diversified objects. In that case a cloudy or wavy film is often seen to pass down. The plausibility of the theory,

[1] *Op. cit.*, S. 110.

however, vanishes when one bears in mind the cases where a dust film is seen even when the after-effects are projected upon the printed sheets and particularly upon dark shadows or cardboards. This film has been sufficiently described already. It is exactly like fine particles of dust moving rapidly through space. One would not expect fading after-images to be of this nature.

A final objection to the fading after-image theory is as follows: The continued passage of a series of stimuli across the retina fatigues the retinal elements. If the elements can recover between stimulations, a movement will be seen. This is what actually occurs. Now after the stimulation has ceased, it is conceivable that one wave of recovery (fading a.-i.) would sweep across the retinal area. I see no reason why successive waves should do so. And yet this would be necessary in order to secure such temporally extended after-effects as are actually observed.

In the light of what has gone before in this paper, we are therefore led to interpret visual after-movement in terms (so far as the retina is concerned) of a streaming phenomenon which passes across the retina in a direction opposite to the image of the objective movement. I confess my ignorance of what this streaming may be. It may be lymph currents, as Ferree supposes. It may be an electrical phenomenon. The uncertain status of its exact nature cannot, however, overcome the necessity of its postulation.

The theory readily explains the phenomena described: The dust films are the projections into space of the streaming. The s.-a.-m. is due to the invisibility of the film because of the clear bold contours of central vision as compared with peripheral vision. This results in the interpretation of the external objects as moving in a direction opposite to the film and occurs under special circumstances only. In the regular after-movement with the large areas, the film is itself visible and drags the external objects along with it. With small areas the same thing occurs with a minimum of film visibility.

EXPERIMENTAL DATA ON ERRORS OF JUDGMENT IN THE ESTIMATION OF THE NUMBER OF OBJECTS IN MODERATELY LARGE SAMPLES, WITH SPECIAL REFERENCE TO PERSONAL EQUATION

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I. Introductory Remarks

In attempting to estimate the number of a considerable group of objects of the same kind, the observer can seldom state the true number but generally gives one which is either too high or too low. If a series of such estimates by the same observer be considered it may be found that there is no tendency for the errors in excess of the true value to be more numerous or greater in amount than those in defect. In such a case the average of the deviations of the estimates from the true number of objects will be sensibly zero.[1] The individual making the estimates may then be said to have no personal equation. Other individuals, however, may have a definite tendency to err on one side of verity in their evaluations. Such may be said to have a positive or a negative personal equation, as the case may be.

Personal equation is not the only factor which should be taken into account in determining the rank of an individual among a number passing judgment upon the value of any magnitude. An observer with no consistent bias towards over or under valuation may be characterized by very erratic judgment—assigning sometimes a value far in excess, at other times a value far in defect of the actual. Thus consistency or steadiness of judgment is also a characteristic of importance which should be taken into account in the comparison of individuals.

In this paper, I have presented several series of experi-

[1] The mean actually determined by experiment would be o plus or minus a smal amount due to the errors of sampling.

mental data bearing on these questions. Such materials, properly analyzed and interpreted, should be of value to the psychologist. The data, which are a by-product of long routine processes in experimental breeding, are presented solely for their intrinsic value as experimentally determined facts. The arrangement of the material is that suggested by the view point of a quantitative biologist, a biometrician. Comparison, criticism and interpretation are left to those having the necessary training. I trust, therefore, that the professional psychologist will overlook crudities in terminology, and accept the experimental data for what they may be worth when interpreted from his own point of view.

The circumstances leading to the collection of these data were the following:

At various times, I have found it necessary to obtain very large series of countings of bean seeds, either for germination tests or for determining the mean weights for different series. Various considerations (which need not be reviewed here) led to the conclusion that the counting could most easily, accurately and advantageously be carried out in units of 25, 50 and 100 seeds. By the slightest addition to our labor we could determine the accuracy of judgment in the estimation of these lots of 25, 50, 100 or 200 seeds. The advantages of so doing were threefold: (*a*) It gave a means of testing the personal equation and the steadiness of judgment of the three assistants who are responsible for a large part of the routine work in my laboratory. (*b*) Competition (for first place in accuracy of estimation) added a little spice to a long task which would otherwise have been the most monotonous drudgery. (*c*) It gave an extensive series of data on errors of judgment.

II. Experimental Methods

The method of the experiment was as simple as can be imagined.

The observer took from a container a handful of beans and poured as nearly as possible a specified number of seeds (25, 50, 100 or 200 according to the experiment) on the table.[1]

[1] The seeds were poured upon a dark gray felt paper mat. This was chosen

If there appeared to be too few, more were added, if too many, a portion were put back. The work was carried out so rapidly that there was no possibility of counting assisting in the estimate.[1] The number was then at once determined by counting and the deviation of the sample from the desired value was noted and recorded by the observer who made the estimate. A persistent effort was made by each observer to improve in succeeding estimates. The influence of this constant checking upon personal equation and upon steadiness of judgment seems to me the most interesting phase of the present study. It will be considered in a subsequent paper.

Except in one special case fifty estimates were made by each observer in the morning and another fifty in the afternoon. In general, this was attended to early in each half day's work. Each lot of fifty may be designated as a period. The periods were, with a single exception, consecutive except for a break over Sunday. Ample details concerning the individual experiments are given below. Four individuals took part in the work.[2]

The following series of experiments were made:

A. Our first series of experiments was made in May, 1912. The attempt was in every instance to lay out a sample of fifty seeds. Observer *A* made only 141 estimates and these with

because the color selected was easy for the eye although affording sufficient contrast with the (generally) white seeds and because the seeds do not roll about as badly on the felt surface. These conditions count for rapidity and comfort of work. Each of the observers occupied her own table, so that light and other conditions were perfectly familiar through long experience.

[1] The validity of this statement will be apparent from the fact that in the first experiment the average time required for pouring out the samples of fifty seeds, counting them twice, recording the deviation of the guess from the true number, and replacing slightly wrinkled or weevil eaten seeds by perfect ones was 86 minutes for Observer *B*, 83 minutes for Observer *C* and 80 minutes for Observer *D*.

[2] My own observations were too few and too continually broken into by extraneous matters to be of particular value. The other three are Miss Edna K. Lockwood, Miss Margaret G. Gavin and Miss Lily J. Gavin. Each of them has been with me for six years or more. It would be difficult to express adequately my obligation to them for their patient, conscientious and highly efficient assistance in the onerous routine, observational, clerical and arithmetical work of a biometric laboratory. For convenience the observers are designated hereafter by letters: Mr. Harris, Observer *A* or merely *A*; Miss Gavin, Observer *C*; Miss Lockwood, Observer *B*; Miss Lily Gavin, Observer *D*.

numerous interruptions. Observers *B–D* made each 700 attempts at laying out the required number. These are grouped in 14 periods of 50 trials each. In the case of *B* and *C* these were made in the mornings and afternoons of seven days which were consecutive except for Sunday. Observer *D* worked on the same schedule but was necessarily absent one afternoon, hence the 14 periods were distributed over 8 days. The data of this series will be designated as I*A*, I*B*, I*C*, and I*D*, the Roman numeral referring to the series and the letter to the observer.

B. The second series of experiments was made in November, 1912. Again the White Navy bean seeds were used and the attempt was to lay out samples of fifty seeds each. The work covered a period of two weeks, in which there was a morning and an afternoon period of 50 estimates each. Thus there were altogether 1,200 estimates by each of three observers, *B*, *C*, *D*. The estimates logically fall into two major periods of six days each, separated by Sunday. They are therefore designated as series II. and III. The appended letters designate the subjects, *B–D*.

C. The experiments were again taken up with the White Navy beans in February, 1913. Again two weeks were devoted to the work and 1,200 trials, in daily morning and afternoon periods of 50 each, were made by the three individuals *B–D*. This time 100 instead of 50 seeds was the number aimed at in the laying out of the samples. The first of the two weeks may be designated by IV., the second by V.

D. The last four days in July, 1913, trials at estimating 200 seeds were made by *B*, *C*, and *D*. Four days' work of 50 trials per day completed the countings that were necessary for the masses of Navy seeds then to be weighed. Because of the time required for the counting and recounting of samples of 200 seeds it was not feasible to do more than 50 estimates in a day. These were made consecutively and usually required a full half day's concentrated work. Doubling the number would have meant an abnormal mental and physical effort for those making the estimates. The experiment is designated as VI.

E. The Tuesday following the preceding set of estimates, (Aug. 5) which were closed on Thursday, a set of trials at laying out 25 beans, of a larger-seeded brown bean—"Ne Plus Ultra"—was begun. Sets of 50 estimates in the morning and afternoon were made daily except for Saturday afternoon (Aug. 9). Thus there were 9 periods each by *C* and *D*. These will be referred to as VII.

F. The week immediately following the preceding trials (Aug. 11–16) Observer *D* did one full week (12 periods of 50 estimates each) on the "Ne Plus Ultra" seeds; again the attempt was to lay out 25 seeds. Series VIII.

G. For the two weeks beginning August 25, 1913, Observers *B* and *C* made trials at the estimation of lots of 25 seeds, using a White bean somewhat smaller than the Navy on which the main bulk of these experiments was based. The first week embraced 11 periods, Saturday afternoon being out. The second week included only 9 periods, Sunday and Monday (Labor Day) separated the two lots. The estimates were closed with Saturday morning of the second week, when the supply of seeds which required weighing was exhausted. Note that Observer *B* had made no estimates since the end of July when she was estimating at lots of 200 seeds. Observer *C* had made no estimates since August 9, when she was working with large seeded brown beans, and estimating at fifties. The first week forms series IX. and the second X.

H. During the period of May 4 to May 9, 1914, inclusive, Observers *B–D* made estimates twice daily at 50 seeds of a large brown bean, Burpee's Stringless. Thus these estimates were made after a lapse of several months (August, 1913–May, 1914) since the last trials. For convenience these will be designated as Experiment XI.

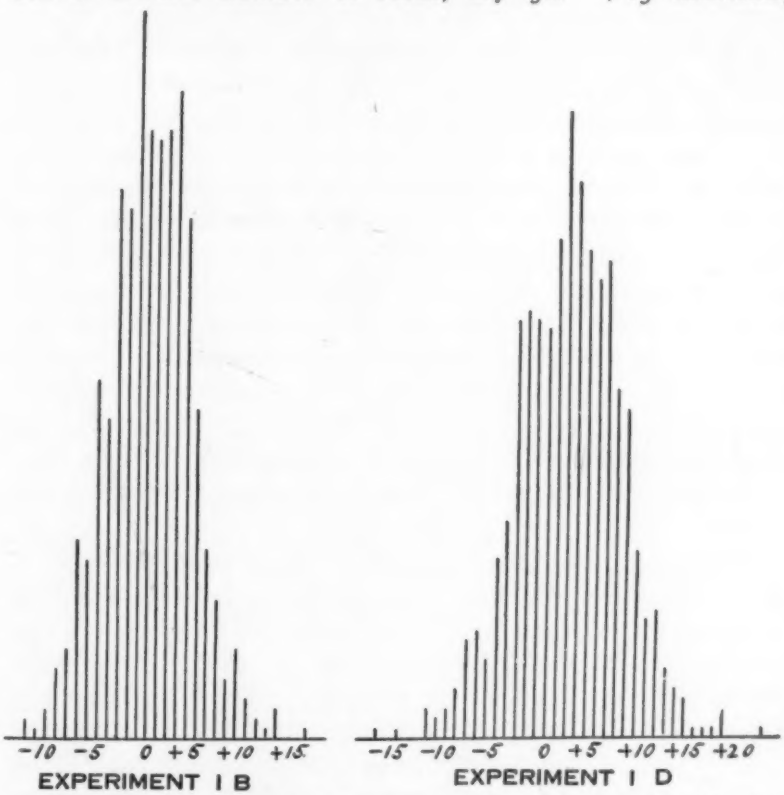
Thus there are altogether 28 sets of experiments, carried out by three observers, distributed over a period of two years, and comprising a total of 15,200 estimates.

The analysis of the data is carried out by the modern higher statistics, the notation of which is very generally familiar or easily accessible.

III. Presentation and Analysis of Data

1. *Personal Equation*

Data Tables *A–E* give the deviations of the number of seeds actually laid out from the desired number (25, 50, 100 or 200), *i. e.*, seeds actually drawn *less* seeds intended to be drawn. Note that the attempt was in each case to lay out a definite number of seeds, say 50. +15 indicates, therefore, that 65 instead of 50, and −6 indicates that 44 instead of 50 were actually drawn.



Diagrams 1–2. Distribution of Errors of Estimation in Experiment I. The heights of the ordinates indicate the frequency of deviations of different grades.

Certain of these series are also represented graphically in Diagrams 1–2.

Two distinct problems are presented by these distributions and graphs. The first is that of personal equation properly so called; the second is that of steadiness of judgment.

By personal equation, we understand a bias in a given direction—a tendency to estimate too high or too low. If there be a personal equation, one observer will tend regularly to pour out too many seeds, just as another will tend to make the sample too small. By steadiness of judgment, we mean consistency in estimation. One observer may be more erratic than another, estimating now far too high, now far too low.

These points may to some extent be illustrated by the first two diagrams. We note that for Observer *B* the frequencies (represented by the heights of the bars) of the grades above and below 0 are about equal, while in the case of the Observer *D* the frequencies above 0 are distinctly greater than those below . Indeed in the cases of *D*, there are six groups of errors of observation above 0 which contain more cases than any class below 0. Apparently Observer *B* has little personal equation, while *D* has a pronounced tendency to lay out too many seeds—that is to underestimate the number of objects in a group. The diagrams also show somewhat the relative steadiness of judgment of the two observers. The deviations appear to be less widely scattered about the mean in the case of *B* than in that of *D*—judgment is apparently steadier, less erratic.

Numerically the existence of personal equation may be most simply tested for by determining the relative numbers of estimates in excess and in defect of the true value. For convenience the tables have been broken up into three compartments. At the head the frequency of cases in which the error was 0 (*i. e.*, in which the experimenter actually succeeded in laying out the number of seeds desired) is indicated. The frequencies of + and − deviations of various magnitudes are shown side by side in the two parallel columns. The totals of these columns give the data needed in answering in the most rough and ready manner the question as to the existence of a personal equation.

Of the 28 experiments made, the totals of the tables show that in only 3 cases is the frequency of minus deviations greater than that of plus deviations. Or in other words, in 25 cases

out of 28 the experimenters *in the long run* made the error of laying out too many seeds.[1]

In comparing the frequencies of + and − deviations of the same magnitude (which have been placed in the tables in parallel columns to facilitate such comparison) it is also clear that in almost every grade of deviation represented by a material frequency the results in excess of the attempted number are more numerous than these in defect.

With results indicating in so striking a manner the existence of a pronounced personal equation on the part of the observers, the calculation of any probable error seems superfluous, especially since probable errors are given for a subsequent test.

A measure of the magnitude of personal equation as well as a demonstration of its existence and direction must be sought. This is most simply expressed in terms of the mean deviation of the estimates from the desired value.

Since in our experiments the attempt was made to lay out a sample of a given size, an excess of plus deviations either

TABLE I

PERSONAL EQUATION FOR INDIVIDUAL EXPERIMENTS

Experiment	Trials	Number Sought	Observer *B*	A/E_A	Observer *C*	A/E_A	Observer *D*	A/E_A
I.	700	50	+.171±.107	+ 1.60	+ .926±.106	+ 8.74	+2.170±.133	+16.32
II.	600	50	+.510±.098	+ 5.20	+1.023±.104	+ 9.84	+ .592±.121	+ 4.89
III.	600	50	+.510±.080	+ 6.38	+ .560±.089	+ 6.29	− .368±.114	− 3.23
II.+ III.	1,200	50	+.510±.063	+ 8.10	+ .792±.069	+11.48	+ .112±.084	+ 1.33
IV.	600	100	+.785±.188	+ 4.18	+1.043±.197	+ 5.28	− .212±.210	− 1.01
V.	600	100	+.280±.151	+ 1.85	+ .893±.156	+ 5.72	+ .890±.172	+ 5.17
IV.+ V.	1,200	100	+.532±.121	+ 4.40	+ .968±.126	+ 7.68	+ .339±.136	+ 2.49
VI.	200	200	+.805±.057	+14.12	+1.960±.058	+33.78	+3.055±.077	+39.68
VII.	450	25	.....	.....	+ .280±.062	+ 4.52	+ .751±.098	+ 7.66
VIII.	600	25	.....	.....	.....	.....	+ .205±.064	+ 3.20
IX.	550	25	+.805±.067	+12.01	− .060±.058	− 1.03	.....	.....
X.	450	25	+.433±.064	+ 6.77	+ .167±.055	+ 3.04	.....	.....
IX.+ X.	1,000	25	+.638±.047	+13.57	+ .042±.040	+ 1.06	.....	.....
XI.	600	50	+.180±.090	+ 2.00	+ .221±.087	+ 2.54	+ .342±.121	+ 2.83

[1] This is also conspicuously the case in the short series of trials by Observer *A*, whose estimates are not to be discussed in detail.

in number or magnitude, or both, is indicated by a mean error with the positive sign. This will indicate a tendency to underestimate any given quantity, since an actually larger number than that desired was laid out.

Table I. gives the results. Of the 28 means for the individual experiments 25 have the positive sign, *i. e.*, in 25 out of 28 experiments the observers had a tendency to lay out too many seeds.

The results are shown graphically in Diagram 3. Here the signs, frequencies and amounts of the personal equations are shown by the direction and length of the lines. The dotted

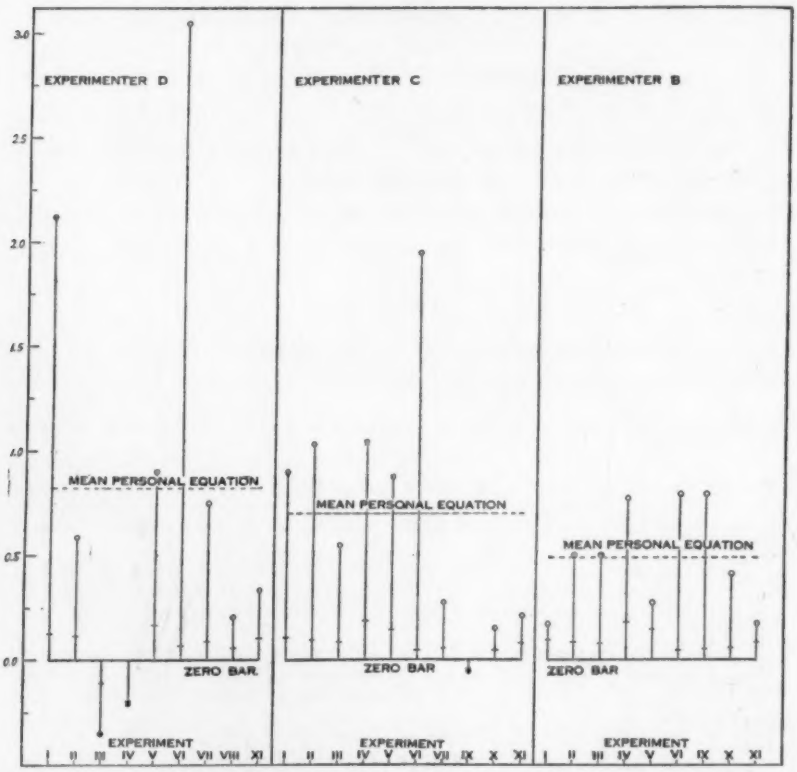


DIAGRAM 3. Sign and Magnitude of (Absolute) Personal Equation in All the Experiments. The mean deviation from zero for the several experiments is indicated by the lengths of the lines. The dotted lines indicate the mean of all the experiments by the individual observers.

transverse lines show the mean values of all the personal equations for the individual observers.

The magnitudes of these means are, however, very low. For Observer *B* there is not a single case in which it amounts to one seed, although in every case it is positive in sign. For Observers *C* and *D* the values are somewhat higher. The largest single value is that of Observer *D*, experiment VI., where the estimates were on an average three seeds off. In this case however there were only 200 trials and the attempt was being made to estimate in lots of 200 seeds.

To the question of the actual magnitude of these deviations from zero personal equation I shall return presently. For the moment, the feature of these results which impresses me is the fact that so slight a personal bias should be so persistent among the three observers throughout the two years during which these observations were carried on.

With regard to the significance of the deviation of these means from 0 little need be said. The probable errors of the mean have been calculated from the usual.

$$E_{\Delta} = .67449 \frac{\sigma}{\sqrt{N}}.$$

Where σ is the standard deviation of the series of errors (to be discussed shortly) and N the number of observations. In Diagram 3 the amount of the probable error is indicated in each case by a cross on the ordinate indicating the amount of personal equation. The ratio of the deviation of the mean from 0 to its probable error has been tabled in each case. Of the 36 constants (including these in which two consecutive experiments have been combined) 28 are over 2.5 times as large as their probable errors. Of these, 27 are positive and one negative in sign. There can be no reasonable question, therefore, of the statistical trustworthiness of these individual constants.

One may test most critically the existence of personal equation by splitting these masses of observations up into sub-classes, say into the groups of 50 estimates known as periods. Constants must then be determined for these in the same manner as for the "general population" as the statisticians call it.

The detailed analyses of one series of data in this way is sufficient, since the others will be treated in a slightly different manner, giving nearly comparable end results, in a subsequent

paper. Table II. for the first experiment furnishes data for determining whether the personal equation demonstrated in the massed estimates is persistent throughout the course of the experiment in the individual periods. In Diagram 4 the solid line in each panel, the zero bar, shows the 0 average of errors of observation which would be secured if there were no

TABLE II

Period	Observer *B*		Observer *C*		Observer *D*	
	Personal Equation	Steadiness of Judgment	Personal Equation	Steadiness of Judgment	Personal Equation	Steadiness of Judgment
1	+1.40	6.24	+ .44	5.34	+1.92	6.85
2	+ .82	5.46	+1.84	4.93	+2.30	7.03
3	− .80	4.10	+1.02	4.13	+3.88	4.89
4	− .56	3.95	+1.02	3.83	+3.02	6.22
5	− .44	3.69	+1.72	4.30	+3.78	5.20
6	+ .10	3.95	+2.04	3.86	+1.36	4.96
7	+ .32	4.23	+1.24	4.05	+2.58	4.75
8	− .72	3.87	+ .50	3.75	+1.22	3.95
9	+ .90	3.79	+1.40	4.22	+1.50	4.34
10	+2.04	3.88	+ .18	4.50	+1.74	4.13
11	+ .20	3.75	+ .50	3.35	+1.04	4.07
12	+ .32	4.11	− .36	3.66	+1.68	3.95
13	− .44	3.46	+1.06	3.70	+ .80	5.42
14	− .58	3.23	+ .36	2.96	+3.56	4.45

personal equation—*i. e.*, no bias towards too high or too low estimates. The circles show the actual means for the 14 individual periods of 50 estimates each. The light line shows the mean deviation for the whole 14 periods, the amount of which is forcibly brought out by the shaded area. The sloping lines show the rate of change. These will be discussed later.

For the whole series of observations, Observer *B* had within the limits of the probable error, no personal equation, *i. e.*, her mean deviation from the true value was only $+ .171 \pm .107$. Here it appears that in 8 of the periods her means fall above and in 6 of the periods below the 0 bar. For Observer C, who appeared from the massed observations to have a distinct personal equation of about one seed, the diagram shows that in 13 out of 14 cases the period means fall on the positive side of the line. Finally, for Observer *D*, who in this series has the greatest bias of the three towards underestimating the number of seeds in a sample—that is, towards laying out too many seeds in the attempt to get 50—all 14 period means are positive.

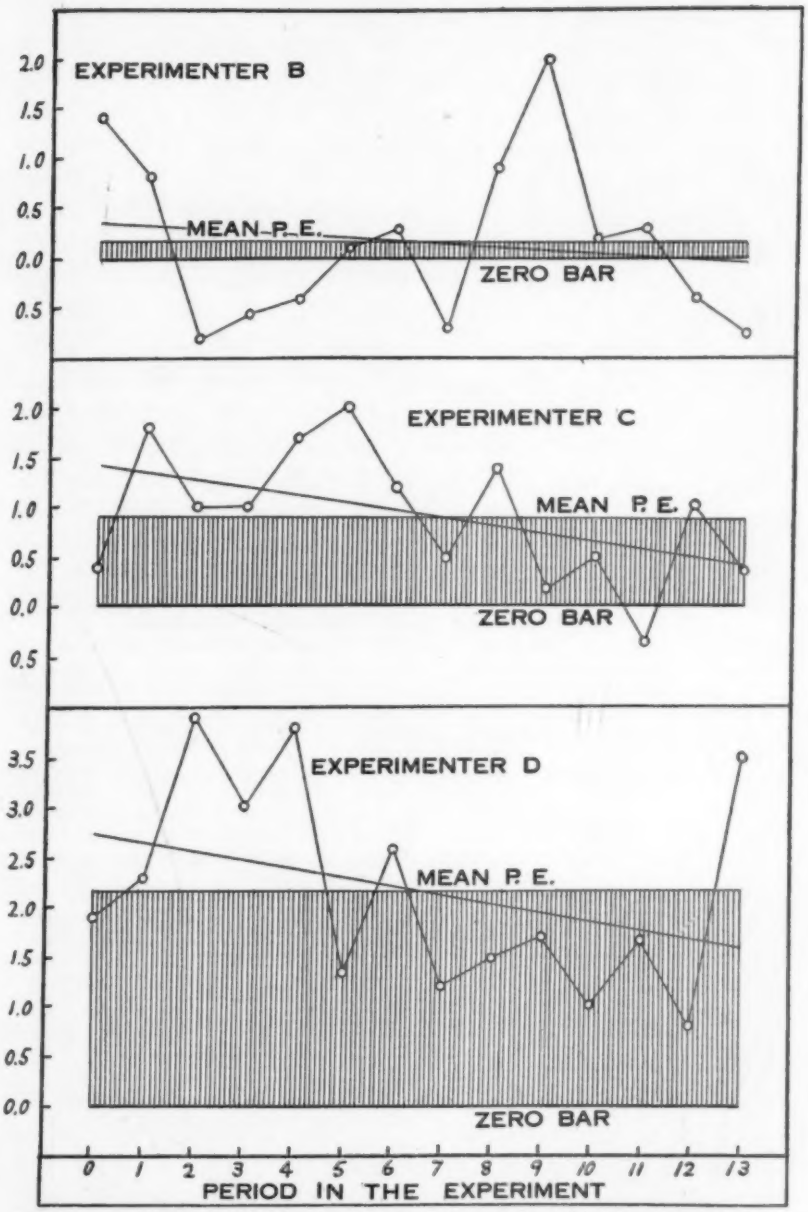


DIAGRAM 4. Personal Equation in the Fourteen Individual Periods, Each of Fifty Trials, of Experiment I. The circles indicate the personal equation for the individual periods. The shaded area shows the amount of personal equation for the whole experiment.

With regard to the actual magnitude of the personal equation, it seems reasonable to assume that if there be a tendency to err from the true value in any definite direction, the actual mean deviation observed will be to some degree dependent upon the number of objects with which the experimenter is dealing.

The simplest assumption is that the actual amount of the personal equation in any given case should be approximately proportional to the number which the experimenter is attempting to estimate. On such an assumption (which on more extensive investigation may or may not be found to be borne out by the experimental facts) one may take the ratio of the actually observed mean deviation to the ideal number. Concretely, one divides the measures of personal equation given in Table I. by 25, 50, 100 or 200 as the case may be. Expressed in this way we have an 'error per object,' or a

TABLE III

RELATIVE PERSONAL EQUATION AND DIFFERENCES IN RELATIVE PERSONAL EQUATION FOR THREE OBSERVERS IN THE INDIVIDUAL EXPERIMENT

Series	Observer *B*	Observer *C*	Observer *D*	*D*—*C*	*D*—*B*	*C*—*B*
I.	+.0034	+.0185	+.0434	+.0248	+.0399	+.0151
II.	+.0102	+.0204	+.0118	−.0086	+.0016	+.0102
III.	+.0102	+.0112	−.0073	−.0185	−.0175	+.0010
II.+III.	+.0102	+.0158	+.0022	−.0136	−.0079	+.0056
IV.	+.0078	+.0104	−.0021	−.0125	−.0099	+.0025
V.	+.0028	+.0089	+.0089	−.0000	+.0061	+.0061
IV.+V.	+.0053	+.0096	+.0033	−.0062	−.0019	+.0043
VI.	+.0040	+.0098	+.0152	+.0254	+.0112	+.0057
VII.	.....	+.0112	+.0300	+.0188	.....	.....
VIII.	.....	.....	+.0082	.....	.....	.....
IX.	+.0322	−.0024	.....	.....	.....	−.0346
X.	+.0173	+.0066	.....	.....	.....	−.0106
IX.+X.	+.0255	+.0016	.....	.....	.....	−.0238
XI.	+.0036	+.0044	+.0068	+.0024	+.0032	+.0008

'relative personal equation.' The resulting values are given in Table III.

The individual entries and the averages show that the relative personal equation is low. The observers tend to lay out about 1 per cent. too many seeds.

Comparisons between the different workers are possible on the basis of these relative values which may be averaged.

From the differences for the individual experiments

(leaving the combined series out of account) the following facts are to be noted.

Observer *B* has a higher personal equation than Observer *C* in 2 cases and a lower personal equation in 7 cases. The average difference between them in terms of relative personal equation is only .0004. Observer *B* has a higher personal equation than *D* in 2 cases and a lower deviation in 5 cases. The average differences, regarding signs as before, is only .0050. A comparison of the records of Observers *C* and *D* shows that in 4 experiments Observer *D* has a greater personal equation than *C* whereas in the other 4 experiments precisely the opposite conditions prevailed. The average difference is only .0040.

From these experimental data taken as a whole one cannot conclude that there is any demonstrated difference between the personal equation of the three observers. All have a bias in the direction of laying out more than the intended number of seeds, but that one is worse than another cannot be asserted.

If now one considers these differences between the personal equations of the three observers in their relation to their probable errors, as shown for the differences in the absolute values given in Table IV., it appears that in a high proportion of the cases they are statistically significant. This is true in

TABLE IV

DIFFERENCES IN PERSONAL EQUATION FOR INDIVIDUAL OBSERVERS

Experiment	*D*—*C*	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	*D*—*B*	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	*C*—*B*	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$
I.	+1.244±.170	+ 7.32	+1.999±.171	+11.69	+ .755±.151	+ 5.00
II.	− .431±.160	− 2.69	+ .082±.156	+ .53	+ .513±.143	+ 3.59
III.	− .928±.145	− 6.40	− .878±.139	− 6.32	+ .050±.120	+ .42
II.+III.	− .680±.109	− 6.24	− .398±.105	− 3.79	+ .282±.093	+ 3.03
IV.	−1.255±.288	− 4.36	− .997±.282	− 3.54	+ .258±.272	+ .95
V.	− .003±.232	− .01	+ .610±.229	+ 2.66	+ .613±.217	+ 2.82
IV.+V.	− .629±.185	− 3.40	− .193±.182	− 1.06	+ .436±.175	+ 2.49
VI.	+1.095±.096	+11.41	+2.250±.096	+23.44	+1.155±.081	+14.26
VII.	+ .471±.116	+ 4.06	.....	.....	.....	.....
VIII.	.....	.....	.....	.....	.....	.....
IX.	.....	.....	.....	.....	− .865±.088	− 9.83
X.	.....	.....	.....	.....	− .266±.084	− 3.17
IX.+X.	.....	.....	.....	.....	− .596±.062	− 9.61
XI.	+ .121±.149	+ .81	+ .162±.151	+ 1.07	+ .041±.125	+ .33

cases in which (for example) *B* has a greater personal equation than *C* as well as in these in which she has a smaller personal equation. The reader may compare the entries in Table IV. for details.

The statement that there are statistically significant differences between two observers for individual experiments, and that these differences are sometimes positive and sometimes negative may be taken at once by that still considerable body of students who are hostile to the newer statistical tools of research to discredit entirely the methods employed and to cast doubt upon the conclusions just drawn. Such an attitude seems to me quite unjustified.

The true interpretation of the results seems to me to be rather that the observers vary somewhat in their personal equation from experiment to experiment, just as they vary from time to time in general health, physiological tone, and mental vigor, alertness, or whatever one may care to call it. As a result of this variation from time to time one observer may show an abnormally high personal equation in a particular experiment in which a second observer shows an unusually low one. On an other occasion the condition may be exactly reversed.

Thus *in an individual experiment* one observer may seem to be decidedly better than another. *In the long run* there is no *fully demonstrated* difference between them.

2. *Steadiness of Judgment*

Steadiness of judgment will best be measured by some expression showing the scatter of estimates around their mean. The best constant for this is the standard deviation, σ.

$$\text{S.D.} = \sqrt{\frac{\text{Sum of (deviations from mean)}^2}{\text{Total estimates}}},$$

which here is most easily calculated from the formula[1]

$$\text{S.D.} = \sqrt{\Sigma(d)^2/N - [\Sigma(d)/N]^2},$$

where Σ is the conventional summation sign, N is the number of estimates and d indicates the deviation of the estimate from

[1] Sheppard's modification has not been applied to the second moment.

the true number of objects, *i. e.*, the actual number laid out less the required number.

The constants with their probable errors are given in the first three constant columns of Table V.

TABLE V

Experiment	Standard Deviation			Coefficient of Variation		
	Observer *B*	Observer *C*	Observer *D*	Observer *B*	Observer *C*	Observer *D*
I.	4.180±.075	4.139±.075	5.211±.094	8.331	8.128	9.989
II.	3.552±.069	3.774±.074	4.407±.086	7.032	7.397	8.711
III.	2.899±.056	3.243±.063	4.137±.081	5.739	6.414	8.335
II.+III.	3.242±.045	3.527±.049	4.301±.059	6.418	6.943	8.583
IV.	6.839±.133	7.160±.139	7.635±.149	6.785	7.086	7.651
V.	5.498±.107	5.682±.111	6.248±.122	5.483	5.632	6.193
IV.+V.	6.210±.086	6.464±.089	6.998±.096	6.177	6.402	6.974
VI.	11.928±.040	12.089±.041	16.172±.055	5.940	5.986	7.964
VII.	.....	1.937±.044	3.072±.069	.....	7.663	11.929
VIII.	.....	.....	2.314±.045	.....	.....	9.179
IX.	2.333±.048	2.013±.041	.....	9.042	8.072	.....
X.	2.010±.045	1.714±.039	.....	7.904	6.813	.....
IX.+X.	2.202±.033	1.887±.028	.....	8.587	7.535	.....
XI.	3.255±.063	3.147±.061	4.399±.086	6.486	6.266	8.738

For steadiness of judgment, we have no absolute standard comparable to a mean deviation of 0 in the personal equation test. The accuracy of an observer must be estimated by comparison with others. A relative measure of steadiness of judgment permitting comparison between different kinds of experiments is desirable. Since it is reasonable to suppose *a priori* that errors of estimation will be larger when the observer is attempting to lay out samples of a large number of seeds than when she is dealing with a small number,[1] this relative measure is best furnished by the biometrican's coefficient of variation, which is obtained in this case by dividing the standard deviation multiplied by 100 by the ideal number plus or minus the observed personal equation, as the sign of the latter may indicate.

These relative measures of steadiness of judgment are given in the last three columns of Table V.

The differences in standard deviation measuring steadiness

[1] Should the number be made very small indeed there would be practically no error of estimation after a little experience, since the observer could all but invariably lay out the correct number.

of judgment between the three observers are set forth with their probable errors and their ratios to their probable errors in Table VI.

TABLE VI

DIFFERENCE IN STANDARD DEVIATION FOR INDIVIDUAL OBSERVERS, THAT IS, DIFFERENCE IN STEADINESS OF JUDGMENT

Experiment	$D-C$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	$D-B$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$	$C-B$	$\frac{\text{Diff.}}{E_{\text{Diff.}}}$
I.	+1.072±.120	+ 8.93	+1.031±.120	+ 8.59	−.041±.106	− 0.39
II.	+ .633±.113	+ 5.60	+ .855±.110	+ 7.77	+.222±.101	+ 2.20
III.	+ .894±.102	+ 8.76	+1.238±.098	+12.63	+.344±.084	+ 4.10
II.+III.	+ .774±.076	+10.18	+1.059±.074	+14.31	+.285±.066	+ 4.32
IV.	+ .475±.204	+ 2.33	+ .796±.200	+ 3.98	+.321±.192	+ 1.67
V.	+ .566±.165	+ 3.43	+ .750±.162	+ 4.63	+.184±.154	+ 1.19
IV.+V.	+ .534±.131	+ 4.08	+ .788±.129	+ 6.11	+.254±.124	+ 2.05
VI.	+4.083±.069	+59.17	+4.244±.068	+62.41	+.161±.057	+ 2.82
VII.	+1.135±.082	+13.84	.....	.....	.....	.....
VIII.	.....	.....	.....	.....	.....	.....
IX.	.....	.....	.....	.....	−.320±.063	− 5.08
X.	.....	.....	.....	.....	−.296±.059	− 5.02
IX.+X.	.....	.....	.....	.....	−.315±.043	− 7.33
XI.	+1.252±.105	+11.92	+1.144±.107	+10.69	−.108±.087	− 1.24

The differences for the standard deviations of the three observers are more consistent than those for personal equation. Observer *B* has more erratic judgment than *C* in 4 cases, less erratic in 5 cases. In 5 out of the 9 cases the difference may be considered to be significant with regard to its probable errors. In 2 of the experiments it is Observer *B* who is significantly more variable in estimation, while in 3 cases it is Observer *C* who has the most irregular estimates. These significant differences which differ in sign from experiment to experiment are probably to be explained in the same way as those in personal equation discussed above. The relative steadiness of judgment as measured by the coefficient of variation shows a mean of 6.971 in the case of Observer *B* as compared with 6.946 in the case of Observer *C*, a difference of only 0.025! Thus in the long run there is no discernible difference in the steadiness of judgment of *B* and *C*, although in the case of individual experiments now one, now the other, may be higher.

For the comparison between both *B* and *D* and *C* and *D* the case is quite different. *In every individual experiment Observer D has* a higher standard deviation, or in other words

less steady judgment, than either *B* or *C*. In practically every instance the differences may be considered significant in comparison with their probable errors. The average relative scatter of estimates of Observer *D* as measured by the coefficient of variation is 8.743, a value about 1.80 higher than that of either of the other observers.

III. Recapitulation

The purpose of this paper, and of another on the influence of previous experience upon errors of judgment which is to follow, is the presentation in terms as succinct as possible of the results of a series of experiments on errors of judgment in the estimation of moderately large numbers of objects.

Data Table *A*

Amount of Error	I. *A*		I. *B*		I. *C*		I. *D*		II. *B*		II. *C*		II. *D*		III. *B*		III. *C*		III. *D*	
0	4		61		79		41		62		64		61		76		80		61	
	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−
1	7	—	60	73	64	58	50	42	77	62	73	63	60	58	76	69	77	64	55	48
2	8	9	61	53	63	65	63	43	56	57	65	34	51	58	75	54	53	56	44	61
3	8	9	65	55	58	46	56	42	38	44	49	44	53	43	70	38	56	43	43	47
4	6	3	52	32	44	36	49	22	38	41	51	15	27	34	50	21	38	28	30	51
5	7	9	33	36	42	22	46	18	35	20	39	16	27	22	18	22	35	16	21	32
6	8	1	19	18	32	14	48	8	18	9	22	12	20	14	8	6	17	11	13	18
7	7	6	14	20	17	7	35	11	14	6	14	4	19	9	8	4	10	4	19	13
8	4	3	6	9	15	6	33	10	9	4	12	3	10	6	3	1	4	2	6	16
9	5	1	9	7	7	2	19	6	6	—	7	2	5	5	1	—	1	—	5	5
10	5	3	4	3	7	2	12	3	2	—	2	2	5	1	—	—	3	—	3	3
11	7	—	2	1	3	1	13	2	—	—	1	2	3	1	—	—	2	—	1	—
12	1	—	1	2	2	1	7	3	2	—	1	1	—	—	—	—	—	—	2	2
13	3	1	3	—	4	—	5	—	—	—	1	—	1	—	—	—	—	—	—	—
14	2	—	—	—	1	—	4	—	—	—	—	—	2	—	—	—	—	—	1	—
15	3	—	—	—	1	—	1	1	—	—	—	—	2	—	—	—	—	—	—	—
16	3	—	1	—	—	—	1	—	—	—	—	—	1	—	—	—	—	—	—	—
17	4	1	—	—	—	—	1	1	—	—	—	—	—	—	—	—	—	—	—	—
18	—	—	—	—	—	1	3	—	—	—	—	—	—	—	—	—	—	—	—	—
19	1	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—
20	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
21	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—
22	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—
23	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
32	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	91	46	330	309	361	260	447	212	295	243	338	198	288	251	309	215	296	224	243	296

The experiments consisted in attempts to lay out samples of a definite number of small objects. The number which the observer was attempting to obtain in each sample (25, 50,

100, or 200) was constant for considerable periods. The error of each estimate was at once determined and recorded by the experimenter, who on the basis of these known errors made a continuous effort to improve in accuracy of estimating.

Two characteristics of the series of errors of estimation made by the three observers are here considered—personal equation and steadiness of judgment. By personal equation

DATA TABLE *B*

Amount of Error	IV. *B*		IV. *C*		IV. *D*		V. *B*		V. *C*		V. *D*		VII. *C*		VII. *D*	
0	31		37		27		47		30		47		95		66	
	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−
1	37	34	32	28	31	35	49	42	47	43	35	32	86	69	63	57
2	34	34	32	31	27	30	44	42	46	40	47	36	63	52	54	52
3	34	31	44	27	18	42	36	29	34	37	38	29	28	20	36	25
4	33	31	34	25	23	35	36	28	29	24	33	29	21	9	28	22
5	25	25	20	22	22	31	27	28	33	30	34	27	4	1	16	4
6	20	29	30	20	31	35	23	30	31	19	29	16	1	1	8	1
7	20	17	14	24	24	17	16	22	23	21	20	16	—	—	6	—
8	12	22	19	10	13	19	10	12	25	16	20	15	—	—	4	—
9	29	10	17	14	9	18	11	18	14	10	12	12	—	—	3	—
10	10	7	21	8	10	16	11	2	7	5	9	6	—	—	1	—
11	9	8	14	12	8	9	10	6	4	2	5	4	—	—	1	—
12	6	5	11	6	9	6	3	4	7	3	13	5	—	—	1	—
13	11	5	8	4	7	5	3	1	10	1	3	2	—	—	—	—
14	3	3	6	1	9	2	2	1	2	1	6	2	—	—	—	—
15	6	3	7	6	4	4	3	—	—	1	1	3	—	—	1	—
16	4	1	3	1	—	2	3	—	1	—	1	2	—	—	—	—
17	4	1	3	—	4	—	1	—	1	—	6	1	—	—	—	—
18	1	—	2	1	1	2	—	—	1	—	1	—	—	—	—	—
19	4	—	1	1	2	1	—	—	—	—	—	1	—	—	—	—
20	—	—	—	1	2	—	—	—	—	—	1	—	—	—	1	—
21	—	—	—	—	3	1	—	—	1	—	1	—	—	—	—	—
22	—	—	1	—	—	—	—	—	1	—	—	—	—	—	—	—
23	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—
24	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—
25	—	—	1	—	—	3	—	—	—	—	—	—	—	—	—	—
27	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—
32	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—
	303	266	320	243	259	314	288	265	317	253	315	238	203	152	223	161

we understand a bias in a given direction—a tendency to estimate too high or too low. By steadiness of judgment we mean consistency in estimation as measured by the closeness with which the errors of estimation clusters around their mean value.

Personal equation is measured by the mean (regarding signs) of the deviations of the samples from their ideal value.

Steadiness of judgment is expressed in the absolute terms of the standard deviation of the errors of estimation about their mean, or in the relative terms of the coefficient of variation.

In the case of all three observers there is a slight but significant personal equation, which, notwithstanding the

DATA TABLE *C*

Amount of Error	VI. *B*		VI. *C*		VI. *D*	
0	6		8		1	
	+	−	+	−	+	−
1	7	5	3	7	8	3
2	8	5	4	8	6	5
3	3	7	1	4	6	4
4	—	4	10	5	7	2
5	5	8	5	3	6	4
6	8	2	10	3	7	4
7	8	1	5	8	5	4
8	3	3	5	—	5	2
9	8	4	10	4	4	2
10	9	4	9	2	7	5
11	6	5	8	6	5	—
12	5	3	2	2	5	5
13	1	5	2	2	9	6
14	4	2	6	4	4	3
15	3	2	4	3	1	—
16	6	4	3	1	2	3
17	1	1	5	2	2	2
18	2	—	2	4	4	1
19	3	1	3	2	4	3
20	2	2	—	—	—	2
21	2	1	—	1	3	3
22	2	2	2	—	3	2
23	2	—	1	1	2	1
24	2	5	2	1	—	2
25	—	1	1	1	3	2
26	3	—	2	—	—	1
27	1	2	2	2	2	1
28	—	3	1	—	2	—
29	—	—	—	2	2	1
30	—	2	1	—	3	—
31	—	1	—	—	—	—
32	—	1	—	—	—	—
33	1	—	—	1	1	—
34	—	1	—	—	—	—
35	—	—	1	1	1	1
36	—	—	—	—	—	—
37	1	—	—	1	1	—
38	—	—	1	—	—	1
39	—	—	—	—	—	—
40	—	—	—	—	1	—
42	—	—	—	—	1	1
43	1	—	—	—	—	—
71	—	—	—	—	1	—
	107	87	111	81	123	76

constant effort to improve, persisted throughout the two years during which the experiments were intermittently made. In only three out of the twenty-eight experiments did the observer lay out samples of too small average size. In a large number of the individual experiments the personal equation is certainly statistically significant (trustworthy) in comparison with its probable error.

It is impossible on the basis of the present series of experiments, extensive though it is, to assert that the personal

DATA TABLE *D*

Amount of Error	VIII. *D*		IX. *B*		IX. *C*		X. *B*		X. *C*	
0	103		83		116		82		109	
	+	−	+	−	+	−	+	−	+	−
1	97	97	91	70	91	104	74	72	99	82
2	79	71	94	52	50	68	79	45	48	48
3	41	35	56	24	34	38	34	23	28	19
4	28	20	35	13	22	16	23	9	9	4
5	8	7	13	2	5	3	7	1	2	—
6	8	1	11	1	1	2	1	—	—	1
7	1	2	3	—	—	—	—	—	1	—
8	1	—	1	—	—	—	—	—	—	—
9	1	—	—	—	—	—	—	—	—	—
10	—	—	1	—	—	—	—	—	—	—
	264	233	305	162	203	231	218	150	187	154

equation of any one of the three observers is on the whole higher than that of the others, although the figures do suggest that the bias of observer *D* may be slightly greater than that of either of the others. In the case of individual experiments there may be significant differences between two observers. In one experiment *x* may have a decidedly lower personal equation than *y*, while in another period of observation exactly the reverse condition may be found. This is taken to indicate a variation in the magnitude of the personal equation of an observer from experiment to experiment.

For steadiness of judgment there is no absolute standard comparable with the zero mean deviation of the personal equation. The data show a coefficient of variation about 6.9 per cent. in the case of Observers *B* and *C*, and of 8.7 per cent. the case of Observer *D*, who has a decidedly greater scatter in her estimates—that is a far less steady judgment—than

either of the other observers. Indeed, in every individual experiment her standard deviation is higher than that of either of the two other experimenters.

Thus while there is no certain differentiation among the experimenters in personal equation, they differ distinctly in steadiness of judgment.

For a more detailed consideration of these two characteristics the reader must see the subsequent paper.

Finally, I must emphasize again the fact that these data

DATA TABLE *E*

Amount of Error	XI. *B*		XI. *C*		XI. *D*	
o	71		78		52	
	+	−	+	−	+	−
1	68	72	66	86	55	50
2	57	57	64	59	50	55
3	58	49	48	50	51	40
4	40	28	33	23	40	36
5	23	26	25	23	24	27
6	12	12	13	7	20	22
7	11	6	10	6	17	11
8	4	1	2	2	11	10
9	1	2	2	1	7	4
10	—	1	—	—	5	1
11	—	—	1	—	3	1
12	1	—	—	—	3	—
13	—	—	—	—	1	1
14	—	—	—	—	—	—
15	—	—	1	—	—	—
16	—	—	—	—	2	—
19	—	—	—	—	—	1
	275	254	265	257	289	259

are presented purely for their intrinsic value. They were secured quite incidentally in the carrying out of large plant breeding experiments. The chief value of the observations perhaps lies in the fact that they represent far larger experiments than the average professional psychologist is able to make. Comprising as they do 28 experiments due to three observers all of whom carried on the work at considerably separated intervals over a period of two years, during which they made over 15,000 estimates, the constants have a reliability which cannot possibly be attributed to short series. Purely psychological discussions, even the review of literature with some of which the writer is quite familiar, is left to specialists.

ORIGIN OF HIGHER ORDERS OF COMBINATION TONES[1]

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It is well-known that Helmholtz gave three or more different explanations of the origin of combination tones. According to his own statements combination tones may be generated (1) from the clicking action between the hammer and the anvil of the ear, when the primaries are powerful;[2] (2) from the asymmetry in vibration of the tympanum; and (3) from disturbed superposition of vibrations due to some objective connection between the primary periodicities, such as a common windchest found in the polyphonic siren or the harmonium, making the air puffs for each tone weaken periodically the puffs for the other tone. The tones generated by conditions (1) and (2) were called subjective; those by condition (3), objective. Helmholtz did not seem to regard case (3) as a condition of disturbed superposition as demanded by his mathematical explanation based upon vibrations '*so large that the square of the displacements has a sensible influence on the motions*';[3] for he states explicitly of this case that he will 'draw attention to a third case, where combinational tones may also arise from *infinitely small vibrations*.'[4] This is of course an error. Helmholtz admitted that in conditions favoring objective combination tones conditions (1) and (2) were also operative, thus making all audible objective combination tones also largely 'subjective.'[5] I have suggested[6] that conditions (2) and (3) are practically identical physically

[1] Read before the Utah Academy of Sciences, April 3, 1915.

[2] 'Sensations of Tone,' p. 158.

[3] *Ibid.*, Appendix XII., 412.

[4] *Ibid.*, 419. Italics mine.

[5] *Ibid.*, 157.

[6] 'Combination Tones,' etc., PSYCHOLOGICAL REVIEW MONOGRAPH, No. 39, 1908, 17 ff., 103 ff.

both being dependent upon the principle of disturbed superposition of vibrations; that such superposition is in all probability in case (2) most pronounced in the liquids of the inner ear. This view has subsequently been supported by Clemens Schaefer.[1]

The view of Helmholtz, expressed under case (1) above, has not been substantiated by recent research. My own experiments lend support to the conclusion of Helmholtz respecting objective combination tones, that they are in large part 'subjective,' *i. e.*, that they are in a considerable measure due to disturbed superposition of vibrations of the primaries within the ear itself.

As to the nature of higher orders of combination tones two contradictory views were expressed by Helmholtz. One was that they are higher order difference tones in the sense suggested by Hällstrom, that they originate from a first difference tone and a primary tone or from two difference tones.[2] His other view was that all higher order combination tones take origin *directly* from the primaries. This is in agreement with a theory developed mathematically in 1881 by R. H. M. Bosanquet,[3] and more recently by Clemens Schaefer in the article referred to in a preceding paragraph. This view that so-called higher order combination tones take their origin directly from the primaries is also supported by the experimental results of a number of recent investigators. From the method of his statement of the laws of the occurrence of difference tones, Krueger has been interpreted by some writers to favor the Hällstrom view of 'higher order' combination tones. This interpretation, which Krueger assured me personally in a conversation[4] is wrong, is used by R. M. Ogden as evidence against Krueger's theory of consonance. Ogden writes that "Stumpf's investigations indicate that combination tones are always directly derived from the objective tones, and not from beats, nor, except in the highest ranges of the scale, from one another as Krueger

[1] *Annalen der Physik*, Bd. 33, 1910, 1216–1226.

[2] 'Sensations of Tone,' p. 154.

[3] *Phil. Mag.*, 5th Series, XI., 1881.

[4] At Cleveland, Ohio, December, 1912.

maintains."[1] This is an unfortunate mistake as to Krueger's view, and, as I hope to show in another place, the actual facts really support rather than refute his theory of consonance. As to the facts, I am willing to go farther than either Stumpf or Ogden and assert that there is no satisfactory evidence in existence to support the view quoted above, that 'in the highest ranges of the scale' combination tones may originate from other combination tones. This is a matter of importance in relation to theories of consonance.

Though Stumpf quotes approvingly my own view as to the so-called higher order combination tones, and seems to accept and to use my experimental evidence, he registers his unwillingness to dispute so generally as I have done the possibility of difference tones being derived from overtones of the primaries.[2] No evidence is offered for such difference tones except that Stumpf has perceived, as other investigators have perceived, difference tones lying in pitch between the primaries. Such tones are, of course, easily accounted for on the theory of direct origin from the primary tones on the basis of disturbed superposition; but the fact of their existence is contradictory to theories like that of Max Meyer. Rücker and Edser[3] established the existence of objective intermediate difference tones, and similar 'subjective' tones are to be expected on the view that disturbed superposition is the cause of all combination tones.

While Helmholtz held to the position maintained by Stumpf as to the derivation of certain difference tones from upper partials, experimental evidence indicates that few difference tones, if any at all, have such origin. Though the possibility of such tones is not disputed by theory—for these tones would follow the same laws as combination tones from the primaries—there is, so far as I know, no evidence at all to indicate that audible difference tones derived from upper partials exist. This is not incomprehensible when one takes into consideration the fact that the overtones of the primaries

[1] In a review of Stumpf in *Psychol. Bul.*, 9, 1912, 117.

[2] Stumpf, C., 'Beobachtungen über Kombinationstöne,' *Zeit. f. Psych.*, 55, 1910, 1 ff.

[3] 'Objective Reality of Combination Tones,' *Phil. Mag.*, 5th Series, 39, 1895.

are themselves to a considerable extent 'subjective,' or dependent upon periodicities arising in the liquids of the inner ear.

In the case of the second difference tone, D_2, which is easily perceptible with a number of intervals, is there any evidence that it can be generated by the higher primary tone and the second partial of the lower primary, as suggested by the formula $D_2 = 2l - h$? In the psychological laboratory of the University of Chicago the resonated forks $Ut_4 : Mi_4$ (4 : 5) give an exceptionally prominent second difference tone, 3, on very gentle sounding, *i. e.*, at an intensity of the primaries which leaves the first difference tone, D_1, entirely inaudible.[1] D_2 can evidently not arise from the first difference tone and the lower primary. On substituting Ut_5 for Ut_4—sounding gently Ut_5 with Mi_4—one should make the tone D_2 even more prominent, if it depends upon the tone corresponding to Ut_5, by the formula $4 \cdot 2 - 5 = 3$. But with such substitution a much greater intensity of the primaries is required to make D_2 (3), now D_1, audible at all. The first upper partial of piano tones in the middle of the register is easily perceptible even to the unaided ear—as are several others of the higher partials. In the case of the major third (4 : 5) with the following tones $c^2 : e^2$, $b^2 : e\flat^2$, and $d^2 : g\flat^2$ the respective second difference tones at approximately the pitch of g^1, $g\flat^1$ and a^1 were easily audible, though the several first difference tones were inaudible. But when the octave of the lower tone in each pair was substituted for this tone (*i. e.*, c^3 for c^2, b^3 for b^2, d^3 for d^2) the difference tone in question in each case became inaudible even though the intensity of the primaries remained the same as before the substitution, or was slightly increased. The tone in question was therefore evidently not dependent upon the upper partials.

In the case of forks $Ut_4 : Mi_4$ (4 : 5) the first difference

[1] It is well known to those who have experimented with tones that such things as tuning forks have individualities almost as marked in certain respects as those of persons. Certain pairs of forks of the few sets with which I have become well acquainted give clear difference tones while others of precisely the same pitch, and resonated similarly, give very weak ones or none at all. The absolute pitch of the primaries is also an important factor in determining the presence or absence of combination tones.

tone, 1, is easily audible when the forks are loudly sounded. Is the second difference tone dependent upon this one? The fork Ut_2 gives a tone coincident in pitch with this first difference tone and, on gentle sounding, almost indistinguishable from it in timbre. Even though this is the case, when Ut_2 is substituted for the primary Mi_4, that is when the interval is represented by the forks $Ut_2 : Ut_4$, the second difference tone, 3, now made the first, disappears altogether. This is true whether Ut_2 is sounded gently—to represent the difference tone for which it is substituted—or loudly. It is evident, therefore, that the second difference tone, 3, of the major third in question does not arise from the first with the lower primary, by the formula $l - (h - l) = l - D_1 = D_2$ or $4 - (5 - 4) = 4 - 1 = 3$. If this is true of the second difference tone it is also true of summation tones if they are really difference tones of higher orders.

But there is yet a better test available. In the case of summation tones theoretically explained according to the formulæ

$$h + l = n(h - l) \quad \text{and} \quad h + l = nh - ml$$

a conclusive quantitative test is applicable, one, however, that was first worked out by the writer only a few years ago. In the case of the fifth (2 : 3) the difference tone of the fifth partials (10, 15) coincides with the summation tone (5). The summation tone is not easy to hear unless its origin is objective to the ear. However with the use of resonated tuning forks I found that it was perceptible to the practiced ear when the forks $Ut_3 : Sol_3$, or $Re\flat_3 : La\flat_3$ were sounded a little above medium intensity. As an objective check a number of students trained in experimental psychology were asked to select from among a number of high pitched forks the tones that were audible in addition to the primaries. With the intervals given were also $Fa_3 : La_3$ (4 : 5) and $Ut_3 : La_3$ (3 : 5). The fork representing the summation tone was in each case selected, as well as forks representing certain first upper partials that were easily perceptible to a trained ear. Occasionally I suggested to the subject a wrong fork, but it

was in every case rejected finally. One of the subjects readily sang the note representing the summation tone. There could be no doubt as to the existence of the tone in question.

By tuning an auxiliary tone to very nearly the pitch of these summation tones well-marked beats were obtained. The auxiliary tone is generated by an unresonated fork sounded gently and held close to the ear. After the auxiliary tone is exactly tuned to the pitch of the summation tone, any change in pitch of the primaries is at once noted by a beating between the auxiliary and the summation tone. On this principle it was found possible to determine the true nature of the latter. Take, for instance, the fifth. The primary tones and their respective upper partials are represented by the following numbers:

2, 4, 6, 8, 10, etc.
3, 6, 9, 12, 15, "

Now it is evident that if the tone in question is a true summation tone, lowering one of the primaries one beat per second will also lower it one beat. But if it is really a difference tone it will in this case take origin from the fifth partials of the primaries, *i. e.*, from 10 and 15. In such a case lowering one of the primaries one beat will lower or raise the tone in question five beats per second. But in the practical working out of this test a difficulty was encountered. Lowering the pitch of a primary tone makes the interval imperfect, and the beats of interference prevent a careful study of the summation tone with the auxiliary. To get rid of these irrelevant beats the following procedure was adopted: After depressing one of the primaries one vibration per second, the auxiliary fork was attuned to the summation tone, a change that could be accomplished with certainty. The primary tone was then brought back to its true pitch. The only beats then remaining were those due to the interference of the auxiliary with the summation tone. The demonstration was convincing; there was absolutely no trace of rapid beats, but slow beats with a rate of one per second were heard. If these beats were due to interference between a difference tone of

the auxiliary and a primary with the other primary, as might be argued, one should hear also rapid beats of the auxiliary with this high tone, if the latter originated from the upper partials. It will be remembered that only such summation tones as were audible were used in the test. Besides, rapid beats of five per second are more easily perceptible than slow ones of one per second. No such beats, however, were audible, and the auxiliary was so weak that no difference tones were probably generated; certainly none was perceptible. The summation tones tested were consequently not explicable as difference tones of upper partials, but were real summation tones in the Helmholtzian sense. These experiments, then, all contradict the view that combination tones may originate from upper partials. While they do not show the absolute impossibility of such origin, we must remember that no case to prove the derivation or combination tones from upper partials exists, and that all the audible overtones of musical intervals are likely to a considerable extent intra-aural in origin.

XXIII. PRACTICE IN GRADING AND IDENTIFYING SHADES OF GRAY

BY WARNER BROWN

The following report consists of two parts. The first part deals with the effects of practice in grading during ten sittings in which an attempt was made to arrange fifty gray cards in the order of their apparent brightness. No corrections were made. The result shows that there was no improvement in the accuracy of the arrangement. The second part, on the other hand, shows that in the course of the ten sittings there was a substantial improvement in the ability to remember and recognize four of the fifty cards.

1. Practice in Grading

The experiment was performed twice a week for five weeks. Each worker was provided with two sets of fifty cards 2.5 cm. wide and 6.5 cm. high. The cards were covered with gray paper ranging from nearly white to nearly black. After being shuffled the cards of one set were laid out by the learner in a row according to the following directions:

> The fifty gray cards are numbered on the backs according to an arbitrary system. You are to arrange these cards according to their apparent brightness, with the lightest on the left, the darkest on the right, and the others graded in between. Do not take more than 15 minutes for this part of the experiment. After you have arranged the cards write down, in the table below, the numbers on the backs of the cards in order.

These directions were printed on a record-blank which also contained the table with fifty spaces for entering the numbers found on the cards. A fresh copy of the blank was supplied to the worker at each practice sitting. Emphasis was laid on the fact that the numbers on the backs of the cards were unreliable and that there was no such thing as a standard arrangement of the cards; but at the same time it was confidently asserted that a good record in the later part of the experiment (to be described shortly), depended upon a scrupulously careful arrangement of the cards in order of

brightness, for the amount of error in the memory and recognition test was to be measured in terms of the worker's own scale of brightness, and any irregularities in the scale would tend to increase the score of his errors.

In order to determine whether any improvement in the accuracy of the arrangement resulted from practice it was necessary to discover the correct arrangement of the cards. As the cards differ considerably in color and in character of surface it was thought that an empirical determination of the correct order would be preferable to a physical measurement of brightness in which these disturbing factors would escape consideration.[1]

The empirical determination of the order of brightness of the cards was made from the records of 37 persons. As each worker had made 10 arrangements there were 370 judgments upon which to base the average position of each card. The data from this computation are presented in Table I. This table shows not only the average position assigned to each card (with the mean variation) but also, under the heading "most probable position," the position which each card would occupy in a series of fifty if they were arranged as nearly as possible in order in a single series according to the average judgment. This last arrangement takes account of the fact that while some of the cards have almost the same average position they can not occupy the same position in an actual arrangement.

When the correct position for any particular shade has been determined it is possible to measure the accuracy of the arrangement for any of the ten practice sittings by the amount of the average deviation of the different persons from that

[1] The cards used in this experiment were prepared from a set of 'Hering' gray papers. The numbers given in the first column of Table I. are those found stamped on the original rolls, but it is evident from the data of Table I. that many of these numbers are without meaning, either because they fail to designate the approximate position in the series (compare No. 20, No. 25, or No. 31), or because different numbers are assigned to papers of almost the same brightness (compare Nos. 20 and 28, or Nos. 25 and 32). Enquiry elicited the information from the manufacturers that they made up different shades at different times, according to the demand, and that the different batches were not alike, so that when they sold an entire set the purchaser received portions of different and dissimilar sets.

TABLE I

Arbitrary Number to Designate the Shade[1]	Average Position	Mean Variation	Most Probable Position
1	1.00	0.00	1
2	2.00	0.00	2
3	3.00	0.00	3
4	4.06	0.12	4
5	5.21	0.37	5
6	5.76	0.39	6
7	6.97	0.08	7
8	8.00	0.02	8
9	9.39	0.48	9
11	10.27	0.78	10
10	11.45	1.04	11
12	11.71	0.95	12
13	13.10	0.85	13
14	13.16	0.83	14
15	15.56	0.87	15
17	17.49	1.45	16
16	17.64	1.53	17
18	17.84	1.42	18
19	18.39	1.56	19
23	20.82	1.54	20
21	21.35	1.66	21
24	21.47	1.73	22
22	21.86	1.53	23
20	25.24	1.86	24
28	25.36	1.63	25
29	26.26	1.77	26
27	26.67	1.51	27
30	27.29	1.70	28
33	29.46	1.52	29
26	30.04	1.72	30
32	30.42	1.37	31
25	30.60	1.52	32
35	34.35	1.43	33
37	34.59	1.53	34
34	35.64	1.60	35
39	35.96	1.52	36
40	36.40	1.75	37
36	37.66	1.41	38
31	38.23	1.74	39
38	38.70	1.21	40
45	42.09	1.21	41
43	42.86	0.96	42
44	42.90	1.12	43
41	43.04	1.18	44
42	44.75	1.04	45
48	46.02	0.80	46
47	46.56	0.55	47
46	47.79	0.39	48
49	49.00	0.10	49
50	49.94	0.11	50

position. A larger deviation means a larger number of cards wrongly placed. Table II. shows the average deviation for each of the ten days of practice. Fewer cards were wrongly

[1] See foot-note on preceding page.

placed the first day than any other day. There is not the slightest tendency to reduce the number of misplaced cards as practice advances. These conclusions are the same whether the deviations are calculated from the average positions or from the 'most probable positions' as defined above. Practice does not increase the accuracy of the work of grading the shades of gray.

TABLE II

THE AMOUNT OF INACCURACY IN GRADING THE 50 GRAY CARDS ON EACH OF 10 TRIALS

The average amount of displacement from the true position has been calculated for 37 persons for each of the 50 cards. The 50 figures so obtained have been averaged to give the figures in the table.

Trial	Av. Deviation from the Av. Position of the Card; All Cards Combined	Av. Deviation from the Most Probable Position All Cards Combined
1	0.95	0.93
2	1.04	1.03
3	1.08	1.10
4	1.13	1.13
5	1.11	1.11
6	1.04	1.04
7	1.11	1.12
8	1.06	1.07
9	1.08	1.08
10	1.06	1.07

2. MEMORY TRAINING WITH SHADES OF GRAY

Most experiments intended to demonstrate an improvement of memory with practice require so much time and such an unusual amount of patience that they can not be made use of in connection with an ordinary course of laboratory experiments. The present experiment shows a very substantial improvement of memory, or at least a form of memory, in the course of no more than five hours of actual work and with very little of the effort usually involved in learning. The portion of the printed directions dealing with the test of memory is given herewith; the conditions of work will be made more clear in what follows.

According to the program for each day's work, given below, pick out four cards from the series as you have arranged it, and observe them carefully, without looking at the numbers on the backs of the cards. Put the first set of cards away. Spread out the other set, face up, on the table. Pick out the same four shades, as nearly as you can, from this second set. Mark in the table the numbers found on the backs of these four cards.

PROGRAM

First day.	Pick out the 10th, 20th, 30th, and 40th.
Second day.	" " " 11th, 21st, 31st, and 41st.
Third day.	" " " 9th, 19th, 29th, and 39th.
Fourth day.	" " " 12th, 22d, 32d, and 42d.
Fifth day.	" " " 8th, 18th, 28th, and 38th.
Sixth day.	" " " 13th, 23d, 33d, and 43d.
Seventh day.	" " " 7th, 17th, 27th, and 37th.
Eighth day.	" " " 14th, 24th, 34th, and 44th.
Ninth day.	" " " 6th, 16th, 26th, and 36th.
Tenth day.	" " " 10th, 20th, 30th, and 40th.

The record of the day's work, when filled out, shows: (1) the actual series of grays as arranged by the student; (2) the four shades selected by him in the memory test; (3) the amount of his error for each shade. The amount of the error depends upon the worker's own arrangement of the cards. Suppose he has sought for the 10th, 20th, 30th, and 40th cards, and that the shades which he has picked out in the memory test, as they lay scattered in irregular order on the table, are the 11th, 20th, 33d and 39th, according to his own arrangement as shown in his record and regardless of the true brightness or arbitrary designations of the cards; the errors in this case would be *plus one*, *zero*, *plus three* and *minus one*, or disregarding the direction of error, a total of *five*. The worker was informed at once of the actual numerical amount of his errors, but he was not allowed to look again at the cards themselves. It will have been observed that different shades were used for the tests on different days so that the practice did not consist of learning to recognize certain shades but was confined to the more general process of learning to perform the mental operation of recognizing this kind of thing.

The actual amount of improvement in the memory test was large. Table III. shows the work of three seasons. In 1911 the recognition test was made with the same set of cards that was used in the memorizing by simply shuffling them up. This involved the difficulty that if there were any spots or other marks on the cards studied they might help in the work of picking out. As the cards suffer considerably from wear

Table III

THE DAILY AMOUNT OF ERROR IN RECOGNIZING 4 SHADES OF GRAY OUT OF A SET OF 50

The figure is the average per person of the difference between the position-numbers of the 4 shades which were selected and the position-numbers of the 4 shades which were sought and should have been selected. The amount of error decreases with practice.

Trial	1911, 33 Persons	1913, 37 Persons	1913, Errors on Probable' Scale	1914, 22 Persons
1	15.5	14.8	14.9	14.2
2	13.4	16.4	16.3	11.3
3	12.7	11.8	12.8	9.9
4	8.3	12.3	12.2	9.6
5	9.1	9.6	10.0	7.5
6	8.2	11.6	11.1	9.9
7	8.2	10.5	11.4	6.4
8	9.5	8.6	9.0	9.8
9	5.9	8.2	8.2	8.3
10	5.8	9.0	9.9	10.0

during the practice such spots multiply, and better records can be made with their help at the end of the period of practice than at the beginning. None of the students of the 1911 group realized that he was making much use of these marks but the result shows that this group made a larger apparent improvement than the latter group who were deprived of this help by being required to select the cards from another set. In 1913 this source of error was eliminated. The subject scattered the cards of a second set and selected from among them according to the directions which have been reproduced above.

The experiment of 1914 was designed to eliminate anothe possible source of error. It was felt by many of the worker that the familiarity with the cards which was acquired in the course of arranging them in order of brightness might be responsible for a large amount of the apparent improvement of memory. Accordingly the experiment was so arranged as to reduce the amount of this factor to a minimum. On the first day the procedure was the same as in the preceding season in order that the first test of memory might be made under exactly the same conditions, but after the first day there was no arranging of the cards in order by degrees of brightness.

Instead, the cards were arranged by the subject according to number with their faces down (they were numbered according to the 'most probable order' described above), then the whole set was turned over and the four cards for the memory test were selected. The worker then proceeded to scatter another set and pick out the cards which seemed to be the same, just as in the experiment of the year before. The experiment was identical with the one of the year before in every respect except that the workers were not familiarized with the material through practice in arranging it. Precisely the same opportunities were afforded in 1914 as in 1913 for the employment of special devices, illegitimate as well as legitimate, as helps in choosing the shades.[1]

The scoring in 1914 was on the basis of the average of previous determinations as shown in Table I. under the heading 'most probable position.' In order to make a comparison possible between these results and those of 1913 the latter were rescored on the same basis; that is to say, the error was stated in both cases as the difference in position between the card sought and the card found if they were all arranged in the 'most probable' order. The results obtained for the 1913 experiment according to this method of scoring are not essentially different from those obtained when the scoring was done on the basis of the worker's own arrangement for the day, and are only a little more regular.

The improvement of recognition through practice is no less rapid in 1914, without the additional familiarity which comes from arranging the cards in order, than in 1913 when this factor was fully operative. With this assurance that the factor of familiarity is of no great importance, and with the evidence that the results can be scored satisfactorily on the basis of the learner's own daily arrangements of the cards without undertaking the laborious process of measuring the actual brightness of the different ones, this experiment gives a simple and easily workable demonstration of the improvement of a form of memory through practice.

[1] The most important of these devices is the estimation of the distance from the end of the scale to the lightest or darkest of the four cards to be memorized. It is

comparatively easy to find these cards by simply counting up mentally, as the cards lie scattered at random on the table, from the white or black extreme to the eighth or whatever one is desired. The other two cards are frequently found by grading in between the two extremes. Only half as many errors were made on the lightest shades as on the middle ones and fewer errors occurred on the darkest shades than on the middle ones. There was no general tendency to under- or over-estimate the shades of gray in the memory test, but too light a shade was selected for the lightest and too dark a shade for the darkest, and the same tendency appeared with the other two shades, as may be seen from the following tabulation of the average number of errors per card.

	Too Light	Too Dark
Lightest card	1.08	0.77
Second card	1.90	1.57
Third card	1.72	1.80
Darkest card	1.17	1.52
Average all four cards	1.47	1.42

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